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ABSTRACTS

Rational Deterrence Theory and Comparative Case Studies

By C. H. ACHEN and D. SNIDAL

Several recent books have argued that comparative case studies of crises demonstrate the failure of rational-deterrence theory; they have offered certain empirical generalizations as substitutes. This paper shows that such contentions are unwarranted. First, the empirical generalizations are impressive as historical insights, but they do not meet the standards for theory set out by the most sophisticated case-study analysts themselves. Second, the "tests" of rational deterrence used in the case studies violate standard principles of inference, and the ensuing procedures are so biased as to be useless. Rational deterrence, then, is a more successful theory than portrayed in this literature, and it remains the only intellectually powerful alternative available.

Case studies are essential to theory building: more efficiently than any other methods, they find suitable variables, suggest middle-range generalizations for theory to explain, and provide the prior knowledge that statistical tests require. Their loose constraints on admissible propositions and suitable evidence are appropriate and even necessary for these tasks. These same characteristics, however, inevitably undermine all attempts to construe case-study generalizations as bodies of theory or tests of hypotheses.

Deterrence and Foreign Policy

By A. L. GEORGE and R. SMOKE

Achen and Snidal's deductive theory of deterrence contributes very little to an understanding of the uses and limitations of deterrence strategy as an instrument of foreign policy. Lacking operationalization, their "rational deterrence theory" is incapable of predicting the outcome of individual cases. Furthermore, it has not yet addressed the need (1) to reconceptualize the problem of deterrence for different levels of conflict; (2) to refine the assumption of "rationality"; (3) to deal with the phenomenon of equifinality; (4) to develop a framework of strategic interaction between Initiator and Defender acknowledging that an Initiator often has multiple options for challenging the status quo from which to choose an action that meets his cost-benefit criteria; (5) to find a way of taking into account decision-making variables that, as case studies have demonstrated, often affect deterrence outcomes; and (6) to broaden the conceptualization of deterrence strategy to encompass the possible use of positive inducements as a means of discouraging challenges to a status quo situation.

Rational Deterrence: Theory and Evidence

By R. JERVIS

The causes and effects of the use of force raise crucial questions of substance and method. Issues are multiple and often are confused with each other. Thus, while many case-study findings contradict "second-wave" deterrence theory, they are consistent with some rational deterrence theories. Many findings, however, cannot be squared with the assumptions of rationality. Policies are suboptimal and behavior is often inconsistent. Furthermore, the actor's values, beliefs, and calculations are exogenous to rational theories and can only be supplied by empirical analysis.

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Rational Deterrence Theory: I Think, Therefore I Deter

By R. N. LEBOW and J. G. STEIN

Deterrence theories purport to supply the auxiliary assumptions rational choice theories need to predict rational strategic behavior. They generally assume that would-be initiators are (1) instrumentally rational; (2) risk-prone gain-maximizers; (3) free of domestic constraints; and (4) able to identify themselves correctly as defenders or challengers. These assumptions are contradicted by empirical studies that indicate that risk-prone, gain-maximizing initiators are relatively uncommon; that leaders at times calculate as deterrence theories expect, but behave contrary to their predictions; and that the calculus of initiators generally depends on factors other than those identified by deterrence theories. Deductive theories of deterrence are also inadequate because they do not define their scope conditions. Nor can they accommodate deviation by initiators from processes of rational calculation. Rational deterrence theories are poorly specified theories about nonexistent decision makers operating in nonexistent environments.

The Rational Deterrence Debate

By G. W. DOWNS

Although there are important differences between the rational theory of deterrence and the theory of deterrence that is emerging from the psychology and case-study literatures, it is necessary for adherents of both to appreciate the ways in which they complement each other and the problems they share. For example, rational deterrence theory will not describe the way the world works until certain heuristics and biases that can only be discovered through case studies and other inferential methods are either eliminated or integrated into the theory. On the other hand, psychologists and case-study researchers will find it difficult to trace through the implications of their discoveries for strategic behavior until they adopt some relative of formal methods.

Power, Capabilities, and Paradoxical Conflict Outcomes

By Z. MAOZ

It has been persistently demonstrated that militarily superior states that are engaged in wars with militarily inferior ones may end up on the losing side. Unlike the processes by which military capabilities are converted into control over resources, the so-called "paradoxical conflict outcomes" phenomenon is more severe than a lack of correspondence between control over resources and control over outcomes. The paradox of power involves cases in which increased control over resources actually *causes* reduced control over outcomes. Several of the theoretical examples of this paradox are taken from social choice theory. These examples are applied to the analysis of the outcome of the Balkan Wars of 1912-1913 and of the Israeli invasion of Lebanon in 1982. The implications of the power paradox for theories of international politics are briefly discussed.

Reforming a Socialist State: Ideology and Public Finance in Yugoslavia

By S. L. WOODWARD

Studies of state institutions in socialist countries demonstrate the inadequacy of current paradigms of the state. Political reforms within these states—like those examined in the works on Yugoslavia that are under review—present opportunities to develop new approaches. Arguments about the democratizing pressures of economic reform or the attempts to legitimize the state by resolving the national question are contrasted to a view of the state as primarily managing its international boundary. Leaders respond to changes in foreign finance, trade, and security with policy shifts that are institutionalized by altering budgetary jurisdictions and rules about spending decisions in both state and economy. The ensuing political conflicts are bounded by the leaders' Marxian project to socialize the state as well as capital, in effect reducing the scope of the federal budget.

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RATIONAL DETERRENCE THEORY AND COMPARATIVE CASE STUDIES

By CHRISTOPHER H. ACHEN and DUNCAN SNIDAL*

INTRODUCTION

RATIONAL deterrence is a highly influential social science theory. Not only has it dominated postwar academic thinking on strategic affairs, but it has provided the intellectual framework of Western military policy in the same period as well. The theory's success derives largely from its clearheaded logic, which is as persuasive as it is elegant.

Yet rational deterrence theory has been sharply criticized by an impressive array of case-study analysts.[1] Examining historical instances of

EDITORIAL NOTE: The essay by Christopher H. Achen and Duncan Snidal on "Rational Deterrence and Comparative Case Studies" raises fundamental questions of theory and methodology with implications that extend well beyond security affairs. The Editorial Committee invited Alexander George and Richard Smoke, Robert Jervis, and Richard Ned Lebow and Janice Gross Stein to respond to the essay, and then asked George Downs to write a commentary on the rational deterrence debate as a whole. The Editorial Committee would like to express appreciation to all of these authors for contributing to this exchange.

* This article is a revised version of a paper presented at the annual meeting of the American Political Science Association, Chicago, IL, September 1987. The research was carried out under the Program in Arms Control and International Studies at the University of Chicago, and funded by the John D. and Catherine T. MacArthur Foundation and under the Program on International Politics and Security (PIPES) supported by the Pew Charitable Trusts project on Economics and National Security. The order of the authors' names was determined by a coin toss.

For their comments on an earlier draft, we wish to thank Henry Brady, David Collier, Ernst Haas, Christopher Holoman, Peter Katzenstein, David Laitin, Jack Levy, Richard Mansbach, James Morrow, John Padgett, Robert Pape, Bruce Russett, Daniel Verdier, Harrison Wagner, and Stephen M. Walt. Raymond Duvall and John Mearsheimer gave particularly detailed suggestions in spite of profound disagreements with the argument. Our critics have saved us from numerous errors, but our opinions are our own, as are any remaining blunders.

[1] Alexander L. George and Richard Smoke, *Deterrence in American Foreign Policy* (New York: Columbia University Press, 1974); Robert Jervis, *The Logic of Images in International Relations* (Princeton: Princeton University Press, 1970); Jervis, *Perception and Misperception in International Politics* (Princeton: Princeton University Press, 1976); Richard Ned Lebow, *Between Peace and War: The Nature of International Crisis* (Baltimore: The Johns Hopkins University Press, 1981), and "Windows of Opportunity: Do States Jump through Them?" *International Security* 9 (Summer 1984), 147-86; Glenn H. Snyder and Paul Diesing, *Conflict among Nations: Bargaining, Decision Making, and System Structure in International Crises* (Princeton: Princeton University Press, 1977); John D. Steinbruner, *The Cybernetic Theory of Decision*

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deterrence and deterrence failure, they conclude that the theory fails both descriptively and prescriptively. As George and Smoke put it, "the contemporary abstract, deductivistic theory of deterrence is inadequate for policy application," since actual cases exhibit "complexities which in many respects are not addressed by the abstract theory of deterrence."[2] A more extreme critic asserts that the theory is completely nonpredictive: "We found that challenges of commitments were largely independent of whether or not those commitments appeared to satisfy our four conditions for successful deterrence";[3] moreover, "deterrence is inadequate as an explanatory theory of international politics because [of] the growing body of empirical evidence. . . ."[4]

The current state of deterrence studies is therefore rather puzzling. On the one hand, the theory is widely regarded as logically compelling. On the other, the most substantial body of empirical evidence leads to the conclusion that it is seriously deficient. The apparent contradiction between logic and evidence provides the starting point for this paper.

The issues raised have broad relevance for social science generally, since the deterrence literature is in many respects a model of cooperation among analysts of different theoretical perspectives. Devotees of comparative case studies have read the rational deterrence theorists with some care. Deterrence researchers who use statistical methods often cite both the historical-case researchers and the abstract theorists; in turn, both groups sometimes cite the quantitative studies. The intellectual interconnections are imperfect, but in most respects the deterrence field is a single literature.

Thus disagreements about deterrence theory raise not just the usual

(Princeton University Press, 1974). Also see the following chapters in Robert Jervis, Richard Ned Lebow, and Janice Gross Stein, eds., *Psychology and Deterrence* (Baltimore: The Johns Hopkins University Press, 1985): Jervis, "Introduction and Assumptions," 1-12, and "Perceiving and Coping with Threat," 13-33; Stein, "Calculation, Miscalculation, and Conventional Deterrence I: The View from Cairo," 34-59, and "Calculation, Miscalculation, and Conventional Deterrence II: The View from Jerusalem," 60-88.

Many other excellent recent case studies in strategic affairs are excluded from this list, either because they do not deal directly with the theory of rational deterrence (e.g., Barry R. Posen, *The Source of Military Doctrine* [Ithaca, NY: Cornell University Press, 1984]; Richard K. Betts, *Nuclear Blackmail and Nuclear Balance* [Washington, DC: Brookings, 1987]; Stephen M. Walt, *The Origins of Alliances* [Ithaca, NY: Cornell University Press, 1987]), or because they do not criticize it (John Mearsheimer, *Conventional Deterrence* [Ithaca, NY: Cornell University Press, 1983]). In addition, we pay relatively little attention to two other important discussions: Snyder and Diesing (fn. 1), which has been discussed elsewhere (see Duncan Snidal, "The Game *Theory* of International Politics," *World Politics* 38 [October 1985], 25-57); and Richard Ned Lebow and Janice Gross Stein, "Beyond Deterrence—Building Better Theory," *Journal of Social Issues* 43 (1987), 5-71, which appeared after this article was written.

[2] George and Smoke (fn. 1), 503.

[3] Lebow, *Between Peace and War* (fn. 1), 274.

[4] Richard Ned Lebow, "Conclusions," in Jervis, Lebow, and Stein (fn. 1), 203.

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suspicions of too little reading by too many scholars, but also important questions about the possibilities and limits of alternative modes of research. It is this latter set of topics that we wish to address. Why have different methodological traditions in deterrence research come to opposing conclusions? On which topics can each school be trusted? More generally, what is the appropriate allocation of tasks in building deterrence theory and understanding?

Obviously, the implications of what we say are not confined to international studies. Although we know of no other area of political science in which the conundrums are posed so sharply, the differences among subfields derive from stages of intellectual development and accidents of intellectual history rather than from inherent features of deterrence. Thus, although our subject matter is deterrence, we believe that our remarks have wide relevance to the study of politics generally.

Within the deterrence field, our primary attention will be focused on comparative case studies and their limitations rather than on the corresponding and at least equally impressive limits of analytic theorizing or statistical analysis, including our own. Our choice is motivated not by the weakness of the historically oriented part of the field, but by its very strength. The comparative study of history has been among the most exciting developments in deterrence thinking during the past 15 years. The sheer number of these studies, along with the many discussions of them in the professional journals, testifies to their intellectual stature. While the analytic literature is only just arising from its quarter-century of slumber, and many statistical investigations continue to struggle for genuine theoretical impact, studies of the two World Wars, Korea, Vietnam, and the Falklands, along with crises such as Berlin, Quemoy and Matsu, Lebanon, and Cuba have had a powerful effect on how political scientists think about deterrence. Much of what the profession knows about deterrence derives from these cases. For precisely that reason, however, it is important to be clear about what exactly has been learned.

In this paper, we examine the claims of the case-study critics of rational deterrence. To preview our argument, we believe that these studies have been enormously valuable for what they contribute—historical wisdom about the limits of current theory, and empirical generalization to be explained by future theory. But case studies have failed when used for two tasks for which they are not suited—theory construction and theory verification. The failures derive not from peculiarities of the deterrence problem, but from the nature of the methods. The logic of comparative case studies inherently provides too little logical constraint to generate dependable theory and too little inferential constraint to permit trustworthy

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theory testing. Only when yoked closely to deductive theory and to statistical inference, and made to serve their ends, can case studies provide genuine theoretical contributions.

We begin our argument by summarizing the logic that supports comparative case studies and by reviewing their positive contribution to deterrence thinking.

The Theoretical Logic of Comparative Case Studies

Adherents of the case-study school argue that theoretical development through historical generalization provides an antidote to the ahistorical and overly abstract theory of rational deterrence.[5] History yields rich insights into nuance and context that escape the simpler rational models. Case studies are therefore essential if the understanding of deterrence is to be grounded in experience and not just in abstract analysis. The ultimate goal is "an explanatory theory of deterrence that is empirically rather than deductively derived."[6]

The logic of using multiple case studies emerged from the limitations of single cases. Analysis of individual deterrence situations, no matter how skillfully executed, does not necessarily "distinguish between what is unique to the case and what is common to the class of events as a whole."[7] Even though single case studies provide interesting insights, they do not *by themselves* provide clear guidance for generalization to other cases. Cumulation is further frustrated by the typical lack of a common theoretical focus, without which meaningful comparisons are difficult. This dissatisfaction with individual case studies, coupled with a lack of faith in the abstractions of rational theory, creates the need for an alternative approach.[8]

[5] The charge is not that deterrence theory is devoid of any appreciation of empirical materials; the major works (e.g., Thomas C. Schelling, *Arms and Influence* [Cambridge: Harvard University Press, 1966]) are replete with examples from across the millennia. Instead, the concern is with the well-known dangers of facile rational reconstruction of events. Intensive case studies—not attempted by scholars such as Schelling—provide a check.

[6] Stein, "The View from Cairo" (fn. 1), 34; see also Alexander L. George, "Case Studies and Theory Development: The Method of Structured, Focused Comparison," in Paul Gordon Lauren, ed., *Diplomacy: New Approaches in History, Theory and Policy* (New York: Free Press, 1979), 48.

[7] Lebow, *Between Peace and War* (fn. 1), 6.

[8] This sort of transition in thinking is hardly unique to deterrence analyses. It parallels the general shift in the understanding of the methodology of comparative analysis throughout political science. For example, George's discussion of the problem (fn. 6) acknowledges significant debts to Harry Eckstein, "Case Study and Theory in Political Science," in Fred I. Greenstein and Nelson W. Polsby, eds., *Handbook of Political Science* (Reading, MA: Addison-Wesley, 1975), Vol. 7, pp. 79-138; also see Arend Lijphart, "Comparative Politics and the Comparative Method," *American Political Science Review* 65 (September 1971), 682-93, and

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By far the most sophisticated methodological treatment of comparative case studies has been done by Alexander George and his coworkers.[9] They recommend the development of *contingent empirical generalizations*—contingent because they apply only under certain (specified) conditions, and empirical because they are derived from analyses of multiple historical cases. These generalizations are produced by the method of "structured-focused comparisons,"[10] which consists of analyzing different cases in terms of common theoretical concepts while simultaneously placing their diversity in a theoretical perspective. Because the cases are tied together conceptually, the generalizations produced are expected to cumulate and become the basic elements of further theory building. Thus, the goal of general explanatory theory is replaced by the goal of segmental theory, which seeks to identify distinct causal patterns and the conditions under which they occur. In this manner, the multiple case-study approach aims to retain the richness of individual case studies without retreating into ideographic explanation, while simultaneously achieving theoretical generality without becoming lost in abstractions.

What George and Smoke are calling for, then, is a sophisticated version of "middle-range theory," inductively derived if-then propositions that are neither purely descriptive nor derived from more general propositions about human behavior. To be consistent with usage in psychology, economics, and the natural sciences, a more accurate and equally honorable title for the kind of generalization George and Smoke seek would be "law," as in Fechner's Law, Say's Law, or Boyle's Law. But, since social science generalizations of any sort so rarely approach the status of laws, we shall refer to them as "empirical generalizations"; we reserve the word "theory" for its more conventional usage in adjacent social sciences—a very general set of propositions from which others, including "laws," are derived. (For example, the empirical generalization that markets clear in capitalist societies can be derived logically from the propositions of neoclassical microeconomic theory.) The reader who wishes to call empirical generalizations "theory," while giving our version of theory a less honorific title, is free to do so. The name will not affect our conclusions.

Lijphart, "The Comparable-Case Strategy in Comparative Research," *Comparative Political Science* 8 (July 1975), 158-77.

[9] George and Smoke (fn. 1), chaps. 4 and 16; George (fn. 6); and George, "Case Studies and Theory Development," presented to the Second Annual Symposium on Information Processing in Organizations, Carnegie-Mellon University, Pittsburgh, October 15-16, 1982; Alexander L. George and Timothy J. McKeown, "Case Studies and Theories of Organizational Decision Making," *Advances in Informational Processing in Organizations* 2 (1985), 21-58.

[10] George (fn. 6).

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Comparative Case Studies and Deterrence

The case-study community has produced detailed case studies of virtually every major international crisis since the late 1890s. The findings have enriched not just deterrence theory but also a wide range of related pursuits, including the study of military coercion, escalation, and war initiation.

These studies are united by misgivings over the rational theory of deterrence, but divided over what should replace it. In one popular approach, deterrence is seen as a fundamentally psychological process, in which cognition failures, fear, or simple time pressures disrupt the rational calculations assumed by deterrence theory. Different authors emphasize different routes to the failure of rational calculations. The first possibility is that decision makers are simply not able to carry out the calculations required by the theory. That is, elites can grasp the costs and benefits involved, they have a sense of the relevant probabilities, and they may be able to perceive the corresponding values for their opponents with reasonable clarity. But because of tension, fear, fatigue, or other thought-inhibiting forces, they cannot combine these elements in the manner mandated by expected-utility theory.

This view of deterrence failures has been taken by several authors, including Janis[11] Lebow[12] and others, but perhaps most systematically and theoretically by Steinbruner.[13] Working from the vast literature on human cognitive limitations, Steinbruner argues that the rational model of human decision making fails both historical and laboratory tests, and must be replaced by something else. He maintains that "real decision makers can achieve only the faintest approximation of the [rational] theory's requirements, and it is not likely that they do even that without unusual (and hence infrequent) effort."[14] His proposed substitute is "cybernetic decisionmaking," in which people avoid all consideration of long lists of alternatives and potential payoffs. Instead, they use feedback from the environment to adjust their behavior marginally, in the same way that a thermostat can keep a house warm without understanding the chemistry or engineering principles of the furnace.

Another psychological factor inducing the cognitive failure of rational deterrence is misperception. Here the emphasis falls, not on the difficulties of computation, but on the assessment of costs, benefits, and their as-

[11] Irving L. Janis, *Victims of Groupthink* (Boston: Houghton Mifflin, 1972).
[12] Lebow, *Between Peace and War* (fn. 1).
[13] Steinbruner (fn. 1), and Steinbruner, "Beyond Rational Deterrence: The Struggle for New Conceptions," in Klaus Knorr, ed., *Power, Strategy, and Security* (Princeton: Princeton University Press), 103-25.
[14] *Ibid.*, 115.

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sociated probabilities. After all, the complexities of deterrence calculations are often unforbidding: if a country knows that it is likely to lose a long, nasty war in the process, it will probably not seek to press its claims against a rival. The trick is to learn the likelihood that the rival country will fight—and if it fights, how likely it is to win.

Robert Jervis has often expressed the view that misperceptions occur routinely in international affairs:[15] "It is hard to find cases of even mild international conflict in which both sides fully grasp the other's views."[16] Although Jervis never quite says that rational deterrence theory fails due to omnipresent misperceptions, the thrust of his work is such that the theory becomes suspect. In practice, Jervis argues, learning enough about the opponent's intentions to make the requisite calculations will be no easy matter. He proposes a variety of cognitive consistency and other psychological models as alternatives.

Psychological approaches constitute the first school of thought about deterrence failures. Although they use case studies, none of the authors mentioned above employ intensive comparative case studies to make their case. To find the method fully applied, we turn to a second school of critics of rational deterrence, who make quite a different argument. They maintain that, although decision makers may respond to those forces modeled by rational-deterrence theory, they are subject to so many other disturbing factors that the theory will not account for much of what happens. Analysts of this school are less interested in pointing to any one type of mental mistake (misperceptions, missed opportunities, bureaucratic bungling) than to the overall predictive power of the theory. Their implicit criterion is a measure of explanatory fit: if one takes rational deterrence as the forecast—namely, that sufficiently strong, clear, credible threats will deter—how often does the theory predict accurately?

This school of thought is capably represented by George and Smoke,[17] and Lebow[18] makes a strong case as well. They point to historical examples in which the threat of retaliation was clear and credible, yet deterrence failed. Thus, George and Smoke report that in

> all three Berlin cases, the Korean War and the Cuban missile crisis, American policy-makers were surprised by the action the opponent took. In each case American officials had thought the opponent would not act as he did because such action would entail high risks. . . . It is evident that to make the diagnoses needed in assessing situations, the policy-maker cannot work

[15] Jervis, *Perception and Misperception* (fn. 1); Jervis, "Deterrence and Perception," *International Security* 7 (Winter 1982-83), reprinted in Steven Miller, ed., *Strategy and Nuclear Deterrence* (Princeton: Princeton University Press, 1984), 57-84; Jervis, "Introduction and Assumptions," and "Perceiving and Coping with Threat" (both fn. 1).

[16] Jervis, "Deterrence and Perception" (fn. 15), 58.

[17] George and Smoke (fn. 1).

[18] Lebow, *Between Peace and War* (fn. 1).

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on the assumption that all actors operate with the same kind of "rationality."[19]

Rather than take issue with the psychological predictions of deterrence theory, as do Steinbruner and Jervis, these authors put more emphasis on its failure to track crisis outcomes.

All these critics of deterrence theory have their disagreements, of course, and one might be forgiven for thinking that they share only an aversion to rational deterrence theorizing. In our view, however, a more important commonality is their belief that explanation consists of giving an account that matches the historical record of particular cases. In contrast to most rational-deterrence discussions (where the cases used are merely illustrative, if they are mentioned at all), each of the authors mentioned above discusses historical cases at some length, and most of them exhibit detailed case studies, usually more than one, from which their lessons are derived. "Theory" here is empirical generalization, not derivation from general assumptions about how people act; the criterion of excellence is a close fit to historical cases, not analytic power or theoretical surprise.

The Theory of Rational Deterrence

Before going on, we want to reacquaint the reader with the bête noire under discussion—rational deterrence theory. A great many deductive arguments are grouped here. However, the rational deterrence literature is unified by a number of working assumptions about human behavior. Together they constitute, not so much a set of beliefs as an explanatory framework—a set of choices about what will be explained and how.

The first two postulates are common to all rational choice explanations; the last is specific to deterrence theory.

1. *Rational actor assumption.* Actors have exogenously given preferences and choice options, and they seek to optimize preferences in light of other actors' preferences and options. (For example, cases of psychopathological decision making are set aside as unsuitable for the rational actor approach.)

2. *Principal explanatory assumption.* Variation in outcomes is to be explained by differences in actors' opportunities. (Appeals to exogenous changes in preferences, or to norms, roles, or culture, are temporarily or analytically suspended.)

3. *Principal substantive assumption.* The state acts as if it were a unitary rational actor. (Changes in personnel, in decision-making patterns, or in bureaucratic politics are not the explanatory focus.)

[19] George and Smoke (fn. 1), 505.

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Many different models of deterrence have been constructed using these assumptions. What all these models have in common is a concern with what we shall call *the fundamental deterrence problem*—the use of threats to induce the opponent to behave in desirable ways.[20] The concept is as old as realist theory itself. Indeed, at this level of abstraction, neither the nature of the weaponry nor the stakes make much difference in the logic. However, the challenges of postwar American foreign policy forced analysts to clarify the model and strengthen its analytical rigor. Initial interests focused on thermonuclear threats to the superpowers' homelands: Type I or basic deterrence.[21] Later work addressed Type II or extended deterrence[22] and conventional threats.[23] Although the distinctions among these situations are life-and-death matters for policy and refer to completely distinct historical cases, they generate only minor differences in the theory: the fundamental problem of deterrence is central to each of them.

In the simplest version of rational deterrence theory, there are two rational actors, the initiator and the defender. The defender seeks to prevent some action by the initiator. (For concreteness, we will assume that it is an attack on the defender or on a third party.) The initiator moves first, either attacking or not. Then the defender chooses whether to engage in war or to capitulate. All this is common knowledge between the two players. In the politically most relevant version, however, what is not known to the initiator with certainty is the defender's ability and commitment to fight back after the attack.

In accord with the conventional theory of extensive form games, analysis proceeds by working backward from the end of the sequence. Suppose, first, that the defender's threat to retaliate is *credible*. That is, the initiator believes it likely that the option to retaliate actually exists (the defender has the military means to retaliate after an attack and is politically free to do so), and that the defender would find retaliation in his interest if the prize is threatened. Then, if the initiator is deterrable (the threatened punishment exceeds his gains from attacking), he will see that an attack will make him worse off than restraint. Hence the initiator will not attack.

[20] An excellent overview is Patrick M. Morgan, *Deterrence: A Conceptual Analysis*, 2d ed. (Beverly Hills, CA: Sage, 1983).

[21] Albert Wohlstetter, "The Delicate Balance of Terror," *Foreign Affairs* 37 (January 1959), 209-34; Glenn H. Snyder, *Deterrence and Defense* (Princeton: Princeton University Press, 1961); Daniel Ellsberg, "The Crude Analysis of Strategic Choice," *The American Economic Review* 51 (May 1961), 472-78.

[22] For example, Bruce Russett, "The Calculus of Deterrence," *Journal of Conflict Resolution* 7 (June 1963), 97-109; Schelling (fn. 5), chap. 2.

[23] For example, Mearsheimer (fn. 1).

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Conversely, if a deterrable initiator believes that it would very likely not be in the defender's interest to retaliate, or that the defender lacks the means or the will to do so, the initiator will attack. Thus, under conventional rational-choice assumptions, when the attacker is deterrable, successful deterrence turns on the defender's credibility. If the latter can convince the attacker that he has the political and military ability to fight, and that the prize (or his reputation for fighting when challenged) is worth more to him than the cost of the war, then, and only then, will a deterrable initiator be deterred.[24]

This simple model is less brittle than sometimes thought. Its propositions are contingent: if the expected punishment exceeds the gain, then opponents will be deterred. Thus, for example, the model implies that some conceivable punishment would deter, but not that any particular one will, nor even any feasible one. Put more strongly, the model implies that deterrence will fail for sufficiently determined attackers. Not all conceivable opponents are deterrable.[25]

Critics of rational deterrence sometimes write as if failures of *deterrence* were equivalent to failures of *deterrence theory*. Levy, for example, notes, "this is an ad hoc hypothesis which cannot technically be derived from any formal theory of deterrence."[26] The truth is that the theory actually predicts some breakdowns. When deterrence fails because the retaliatory threat is absent, incredible, or less valuable than the prize, the theory has forecast perfectly. Rational deterrence theory implies that deterrence will not always be successful.

Rational deterrence is very much an ideal-type explanation. No sensible person pretends that it summarizes typical deterrence decision making well, or that it exhausts what is to be said about any one historical case. Yet it has dominated discussion in all traditions of deterrence research,

[24] More precisely, the initiator's subjective expected utility of attacking must be less than that of continuing the status quo. Among other factors, his expectations depend upon his subjective estimates of the expected costs of war, the probability of winning, and the estimated probability that the defender will retaliate. The latter, in turn, is a function of the initiator's subjective prior distribution over the defender's utilities in a game of incomplete information.

Analysts continue to struggle painfully for a fully satisfactory version of this game; as Harrison Wagner has remarked to us, "the rational theory of deterrence" doesn't exist. (Morgan [fn. 20, chap. 4], gives a clear exposition of the principal difficulty in the theory.) Hence the grim looks from game theorists when international relations scholars remark that there is no work left to do on rational deterrence. But just as one can believe Newton's laws without waiting for physicists to fully comprehend the nature of gravitation, so also the principal conclusions of a legitimate theory of deterrence are foreseeable even if the supporting arguments are at present incomplete; it is the former that we call "rational deterrence theory."

[25] Schelling (fn. 5), 85-86.

[26] Jack Levy, "Quantitative Studies of Deterrence Success and Failure," paper prepared for the meeting of the American Political Science Association, Chicago, IL, September, 1987, p. 2.

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including historical investigations. For example, George and Smoke criticize rational deterrence for its assumption that a government is a rational actor and can be treated as if it were a single person.[27] Yet the same authors summarize their case-study evidence as follows:

> In almost every historical case examined, we found evidence that the initiator tried to satisfy himself before acting that the risks of the particular option he chose could be calculated and, perhaps more importantly, controlled by him so as to give his choice of action the character of a rationally calculated, acceptable risk.[28]

The rational unitary-actor model is not easily evaded.

The power of rational deterrence theory is conceptual, not mathematical. It derives from the underlying logical cohesion and consistency with a set of simple first principles, not from the particular language in which it is expressed. In consequence, the model has been astonishingly fecund, both for theory and for policy. Its surprising implications, now familiar from the literature, include "the rationality of irrationality," the dangers of total disarmament, and the value of aiming for strategic equivalence between the superpowers.[29] Perhaps most importantly, it was rational deterrence theory that sensitized policy makers to the negative aspect of defensive systems such as civil defense, the ABM, and SDI, which make first strikes less dangerous. The point is strongly counterintuitive; indeed, Aleksey Kosygin told Lyndon Johnson at Glassboro that he didn't understand it.[30] But this surprising conclusion is a clear implication of rational deterrence theory.

Contrary to George and Smoke's view, rational deterrence theory has proved itself in practical policy applications. No other theoretical perspective has had nearly the impact on American foreign policy, certainly not the conclusions of the case-study literature. Far from being an abstract, deductivistic theory developed in a policy vacuum, rational deterrence theory has repeatedly taken inspiration from the most pressing policy questions of the day, from decisions of bomber-basing in the 1950s to SDI in the 1980s. It has set the terms of the debate, and has often influenced the outcome. One may choose to applaud or decry its impact, but one cannot deny that the theory of rational deterrence, like any good theory, has been of immense practical importance.

[27] George and Smoke (fn. 1), 72-77, 505.

[28] *Ibid.*, 527.

[29] Many of these appeared first in the work of Thomas Schelling, notably *The Strategy of Conflict* (Cambridge: Harvard University Press, 1960), and *Arms and Influence* (fn. 5).

[30] Henry Kissinger, *White House Years* (Boston: Little, Brown, 1979), 208.

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Comparative Case Studies and the Quest for Theory

We now must contrast the empirical generalizations of the multiple case-study approach with those of rational deterrence theory. As noted above, the case-study method at its best uses side-by-side comparisons of different deterrence crises to produce contingent empirical generalizations. These generalizations are viewed by some as constituting theory,[31] and by others as potential substitutes for the rationality postulates of deterrence theory.[32] Still others, arguing that abstract theory is infeasible, substitute a long set of possible variables and call the result a "paradigm."[33] In any case, the generalizations are viewed as filling the empirical void opened by rational deterrence theory.

What are the new contingent generalizations or empirically grounded theories that case studies have produced? Examples are not as easily found as one might imagine. Consider, for instance, Lebow's analysis of certain "acute" international crises from 1897 to 1967.[34] He groups them into three categories. The first crisis category is that of "justification of hostility": the initiating state, having already decided that war is in its interest, creates a crisis to provide a casus belli and to mobilize support for the war. The second category includes nine "spinoff" crises that lead to a war secondary to some other conflict. Lebow does not analyze either of these first two categories of crisis in great depth. We would observe only that *all* the outcomes in these categories correspond exactly to rational deterrence theory. War occurred because it was "rational" for one or both states.

The third and final category is that of "brinkmanship" crises; war occurs sooner or later after most of them. Lebow mentions 24 factors that influence the outcome of such crises.[35] He does no full-fledged "process tracing" of comparative historical sequences in the sense that George and Smoke do, but he reviews a great many crises in comparative perspective, and he draws attention to certain key dimensions—for example, domestic politics—which are not currently addressed by deterrence theory. The fact that domestic politics matters has important consequences not only for deterrence, but for realist international-relations theory more generally. Its ultimate implication would be the desirability of including domestic processes in all models of international politics. More immediately, Lebow's hypothesis reminds us that the characterization of states as unitary actors is an analytic assumption, not a truth.[36]

[31] George and Smoke (fn. 1). [32] Jervis (fn. 1).
[33] Lebow, *Between Peace and War* (fn. 1), 304; see also "Conclusions" (fn. 4).
[34] Lebow, *Between Peace and War* (fn. 1). [35] *Ibid.*, 304.
[36] Lebow's later work is suggestive of auxiliary questions that arise from these considera-

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Lebow's work, like that of Steinbruner and Jervis, is typical of the case-study literature in that many of his findings are statements of the form "Factor *X* is a key feature of the international system," or "in the time period under study, Factor *X* was more important than Factor *Y*," where Factors *X* and *Y* are domestic forces, misperception, bureaucratic politics, and so on. Results of this kind are crucial to theory building: learning the important variables is often the most demanding task. Lebow therefore makes a genuine contribution when he reminds us of the domestic factor. Like the other variables he mentions, "it matters," and we need to remind ourselves that it does. Case studies can perform that function admirably.

Lists of variables, however, are not "theory," no matter how one defines that elusive concept. Not much successful theorizing from any methodological standpoint about the effects of domestic politics has been accomplished in international relations—simply because domestic factors add complications that are currently impossible to deal with. (The economists have done little better with the closely related topics of social choice and principal agency.) As a discipline, international relations is nowhere near understanding these aspects of deterrence in a coherent and theoretically rigorous manner. Reminding ourselves that we do not understand them is important, but does not itself solve the problem.

Instead of lists of variables, we must look for plausible empirical generalizations—well-specified causal sequences which can be found across a significant range of historical case studies, which are not already familiar from the long history of international relations thought, and which are not straightforward applications of ideas borrowed from other literatures. Again, we turn to George and Smoke.

In *Deterrence in American Foreign Policy*, they present a comprehensive historical analysis of recent American deterrence policy.[37] They are self-consciously concerned about methodology, and their approach is very close to that in George's "Case Studies and Theory Development" described above.[38] While they offer a wide range of useful insights on individual cases, their most systematic set of contingent generalizations concerns "patterns of deterrence failure." These are presented as a major reformulation of deterrence theory, derived from empirical case studies.

George and Smoke offer a typology of three main sequences for deterrence failure.[39] The first ("the fait accompli") is simply the absence of

tions. He proposes the "general principle . . . that policy makers . . . are probably inordinately influenced by immediate and predictable costs" based on analysis of how the short-term interests of politicians diverge from the long-term interests of states. (Richard Ned Lebow, "Miscalculation in the South Atlantic: The Origins of the Falklands War," in Jervis, Lebow, and Stein [fn. 1], 123-24.)

[37] George and Smoke (fn. 1).

[38] George (fn. 6).

[39] George and Smoke (fn. 1), chap. 18.

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credibility in the deterrent threat. As noted earlier, this breakdown is a clear consequence of rational deterrence, and need not concern us here. The other two are considerably more interesting.

In the second pattern ("the limited probe"), the initiator employs a limited and relatively riskless challenge—for example, China's shelling of Quemoy in 1958. George and Smoke argue that the Chinese were simply interested in learning the definition and strength of the American commitment to Taiwan. By engaging in a low-level and essentially nondeterrable act, they not only learned a good deal about U.S. values and intentions, but also brought the matter forcibly to Washington's attention. In one sense, deterrence failed; in another, it succeeded, because Taiwan was not invaded.

The third pattern ("controlled pressure") may follow the second, or it may be generated independently. In either case, the initiator uses nonmilitary tactics that are not easily countered without military retaliation. Soviet pressure on Berlin in 1958 and in 1961 are examples. Its purpose, Smoke and George maintain, was to create pressure for a solution, divide the Allies and, if possible, erode the American commitment.

George and Smoke argue correctly that the deterrence failures of the second and third patterns are not those envisaged by the conventional rational deterrence theory. We would add that rational deterrence does not contradict these scenarios; it simply does not consider them. In conventional deterrence theory, crises have to do with competitive risk taking and the slippery slope of inadvertent war[40]—a very different process from the one discovered by George and Smoke. To our knowledge, crisis sequences like Quemoy and Berlin have received no attention at all from rational deterrence theorists. Yet they are clearly of major importance for policy, and for theory as well. The discovery of empirical generalizations like these is a considerable achievement, and a success that only comparative case studies are likely to achieve.

In the hands of George and Smoke, the case-study approach helps to generate theory in a very direct way. Indeed, generalizations like theirs are a necessary condition for building relevant theory. Yet we would insist that even in the hands of masters, empirical generalizations do not constitute theory—or at any rate, not the most useful kind of theory for social science. Even for policy purposes, their usefulness is limited. Case studies of crises can provide insights to policy makers, but those insights rarely transcend the particular issues for which they were developed. Unlike rational deterrence theory, scenarios for crises provide no general framework for thinking about the broad range of security issues such as

[40] See, for example, Schelling (fn. 5), chap. 3.

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ABM treaties, SDI, or antisubmarine warfare. Empirical generalizations lack the universality that is the hallmark of good theory. A typology, no matter how novel and insightful, is not an interrelated set of propositions from which powerful and surprising consequences can be deduced.[41]

For theoretical purposes, the difficulty with explaining individual cases is that there are so many details in every case that no single theory can reproduce them all, and *some* evidence can be found for too wide an array of variables and propositions. Even if the goal is the explanation not of individual cases but of causal sequences, the dilemma remains. Since there is no requirement that behavior be logically consistent with any one explanatory framework, there are too many degrees of freedom for explanation. As George and Smoke note, typologies and causal sequences can be multiplied indefinitely, with the number found being partly a matter of how detailed the investigator wishes to be.[42] If explanations are not required to apply to all cases, we can specify contingent conditions to protect any favored generalization, so long as it can be plausibly supported in *some* cases. And the more we learn about each case, the more distinct it becomes. Even in the ideal case, nothing in this approach precludes having one causal pattern per case.

A deductive theory like rational deterrence reduces this problem by providing stricter criteria for admissible hypotheses. Only those that follow directly from the theory are to be considered. This imposes a requirement of logical consistency and interrelatedness among explanations which maintains parsimony and prevents the proliferation of ad hoc hypotheses. Moreover, the theory is universal rather than typological: if more than one causal process can occur (e.g., deterrence success *or* failure), the theory is expected to give conditions under which each applies.

Parsimony has a price, however: deductive power is usually purchased at the cost of historical accuracy. What most deeply separates scholars of the historical-comparative school from the deductive tradition (which in-

[41] The typological historical sequences in George and Smoke seem to us their most important contribution. However, the authors also give prominence to a list of more abstract propositions that derive from their work. Two typical examples:

> Proposition 3: The initiator's belief that the risks of his actions are calculable and that the unacceptable risks of it can be controlled and avoided is, with very few exceptions, a necessary (though not sufficient) condition for a decision to challenge deterrence, i.e., a deterrence failure (George and Smoke, fn. 1, 529).
>
> Proposition 6: Deterrence success will be favored but not ensured by the belief of the initiator that the defender possesses (a) an adequate and appropriate spectrum of capabilities; (b) sufficient motivation to employ them; and (c) probable freedom from impeding political constraints in the relevant time period (*ibid.*, 530-31).

It is not clear to us that propositions of this kind are a challenge, or even an addition, to unitary rational-actor models of deterrence.

[42] *Ibid.*, 535.

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cludes approaches other than rational choice) is the question of trade-offs between analytic power and historical concreteness.[43] Should theory seek middle-range propositions closely related to individual cases? Or should it abstract further away from concrete instances to achieve deductive power? Put more precisely, should theory be applicable to historical instances, so that one or another causal pattern may be attached to each case? Or should it be a ceteris paribus explanation, accounting for an aspect of many cases, but not fully for any, and perhaps not at all for some?

This debate has a long history. In 1883, Carl Menger published the first edition of his *Investigations into the Methods of the Social Sciences with Special Reference to Economics*, an attack on the historical school of political economy.[44] Thus began the famous *Methodenstreit* in late nineteenth-century Austrian academia, pitting Menger against Gustav Schmoller and others. Menger argued for ideal-type rational-choice explanation, in which social facts were explained as consequences of individuals' choices. Schmoller preferred historical explanation at the level of the society, with an emphasis on the study of particular cases.

The dispute between rational-choice theorists of deterrence and their historically oriented counterparts essentially recapitulates the *Methodenstreit*. A debate of this sort is never settled (and perhaps never should be).[45] With respect to deterrence theory, however, the choice seems clear. Rational deterrence has not been seriously challenged theoretically by the comparative case-study school. As the case-study analysts themselves admit, they have not yet produced anything that approaches "integrated theory,"[46] and their analyses are best characterized as a "methodological potpourri,"[47] offering "an elaborate array of hypotheses."[48] These hy-

[43] *Ibid.*, chaps. 3, 4.

[44] Menger, *Investigations into the Methods of the Social Sciences with Special Reference to Economics* (New York: New York University Press, 1985).

[45] Terry M. Moe, "On the Scientific Status of Rational Models," *American Journal of Political Science* 23 (February 1979), 215-43, for example, offers an interesting recent twist that might be profitably read for contrast with our position. He uses the traditional positivist covering-law model of explanation to attack a variety of bad arguments for rational choice models. The main issue, however, is whether rationality models use abstraction in the same way as any other scientific theory does. Moe is brief on this point, but he argues that natural science uses empirically based abstractions, while rational choice builds causal processes in by assumption. Unfortunately, he arrives at this conclusion by comparing rational choice theory, not to a natural science theory, but to an empirical generalization (Galileo's Law). Had he—more logically—compared the causal structure built into rational choice with the causal structure built into Newton's theory (which explains Galileo's Law), his conclusions would have been reversed.

Another approach to the role of formal theorizing, particularly in international relations, appears in Bruce Bueno de Mesquita, "Toward a Scientific Understanding of International Conflict: A Personal View," *International Studies Quarterly* 29 (June 1985), 121-36.

[46] Jervis, "Introduction and Assumptions" (fn. 1), 5; Lebow, *Between Peace and War* (fn. 1), 102.

[47] *Ibid.*, 19.

[48] Lebow (fn. 4), 232.

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potheses provide a better understanding of individual cases than does rational deterrence, along with useful advice for policy makers. At their best, they have also provided certain contextualized generalizations that may become a major step toward deductive theory building. But what George and Smoke call

> the significant aspects of theory—the precision of concepts, the formality and exactitude of their logical relationships, the degree to which the theory can be expressed in mathematical or quantitative terms, the degree of general consensus enjoyed by the major ideas and the relative comprehensiveness of these agreed-upon ideas, the stability of theory over time, and the level of confidence in the theory held generally and especially by policymakers[49]

—these are aspects of theory that have been absent from the case-study literature.

We are not complacent about the current state of rational deterrence theory. For instance, while George and Smoke regard signaling and commitment as the "best developed" part of rational deterrence theory,[50] we consider these elements to be woefully underconceptualized. Most rational deterrence theorists believe that Schelling's "threat that leaves something to chance" has yet to be given a coherent statement within rational-choice theory, and the same is true of NATO's doctrine of "flexible response."

More importantly, our goal here is to defend theory in general, not rational deterrence theory. Rationality postulates and unitary-actor assumptions are always debatable. Rational-choice models explain a good deal in some situations, and not much in others. We agree with the critics that, in spite of exciting recent developments in game theory, there is much to do before rational deterrence models begin to track certain key features of important historical crises, and a good deal of empirical work remains before the overall usefulness of rational deterrence in explaining actual policy decisions can be assessed. Ultimately, better theories based on more realistic assumptions about human decision making may be developed.[51] As we have argued, however, rational deterrence theory is considerably stronger than it has been portrayed in the case-study literature. In the absence of a well-developed theoretical alternative, it would be folly to abandon it for the patchwork of empirical findings currently available.

[49] George and Smoke (fn. 1), 45. [50] *Ibid.*, 65.

[51] When such theories appear, however, they are not likely to emerge directly from the results of case studies. In our view, the notion—common to both case-study and behavioral traditions—that theory emerges from masses of facts and lower-level generalizations, is false both to the history of the natural and social sciences and to the concept of theory.

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The Empirical Logic of Comparative Case Studies

We now turn to the attempt to disconfirm rational deterrence empirically by means of case studies. The logic of "testing" used in the multiple-case approach is again best described by George.[52] Its central element is the "plausibility probe," which assesses various hypotheses by detailed analyses of multiple cases. This approach is different from traditional statistical testing, but complementary to it.[53] The rich details of case studies allow the analyst to look at more than linear correlations between variables across an entire population, and to look for more complex processes in particular contexts. Finally, the use of multiple cases allows for comparisons of causes across contexts.

In practice, however, the empirical testing of rational deterrence by comparative case studies has been much less impressive than George's description would imply. These studies routinely violate standard principles of inference, and the resulting logical looseness makes it all too easy to draw conclusions in accord with the investigator's predilections.

Two of the inferential felonies in the case-study literature are of special importance. First, the selection of cases is systematically biased. Cases are chosen nonrandomly by criteria that make an evaluation of rational deterrence theory impossible. Second, case-study analysts have misinterpreted the propositions of rational deterrence as descriptions of decision makers' thought processes. The resulting "tests" are essentially useless as a judgment on the validity of rational deterrence theory.

To begin with, the nonrandom character of case selection is evident in Lebow as well as in George and Smoke. Both focus on crises which, in one sense or another, are already deterrence breakdowns. For example, Lebow treats 26 "acute" cases, of which all but 3 are either fait accompli crises, "spinoffs" from an ongoing war, immediate precursors to war, or rehearsals for a later crisis that did end in hostilities, such as Bosnia or Munich. Readers looking for happy endings must content themselves with Fashoda, Berlin (1948), and the Cuban Missile Crisis. George and

[52] George (fn. 6).

[53] George (*ibid.*), notes that Bruce Russett, "International Behavior Research: Case Studies and Cumulation," in Michael Haas and Howard Kariel, eds., *Approaches to the Study of Political Science* (San Francisco: Chandler Publishing, 1970), makes a parallel case from a quantitative perspective in favor of case studies for tracing causal patterns and for policy purposes. More recently, Charles Ragin, *The Comparative Method* (Berkeley: University of California Press, 1987) has made similar claims. He argues that, at least as used by most social scientists, purely statistical methods rarely turn up intriguing findings or hypotheses. Social forces are nonlinear ("contextual") and interactive ("holistic"). Hence they are blurred and averaged meaninglessly in "variable-oriented" approaches (or at least in the linear statistical models and casual data analysis in common use). He recommends case studies to find the detailed causal patterns in different contexts.

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Smoke's sample of crises is not very different from Lebow's. Indeed, in hundreds of pages, the reader rarely encounters anything but deterrence failures. The cumulative impression is overwhelming, and the mind tends to succumb.

Let us consider, however, some of the "crises" and "wars" that go undiscussed in this literature. The first Soviet-American War which erupted over Hungary in 1956 is missing, as is the second one (over Chile) in the early 1970s. The U.S.-China War, which began when the United States bombed the North Vietnamese dikes is missing, as is the second Korean War. The point, of course, is that none of these "wars" occurred, or came anywhere near occurring. And deterrence is a likely cause of their prevention.

Dozens of such examples will occur to the reader. But, because the details of successful deterrence cases are rarely discussed in the historical literature (for good and obvious reasons), analysts who want to know how often deterrence fails *and how often it succeeds* can be badly misled by consulting only crises and wars. An example may perhaps make this point clear. Suppose that there are 100 countries in the world, each of them in a deterrence relationship with an average of 5 of the others. Thus, each year there are 250 war-prone dyads. In 40 years of observation, this system provides a total of 10,000 opportunities for war. Suppose that all the countries are deterrable, and that sufficiently strong, credible threats have been communicated in each case. In this rather special world, rational deterrence theory predicts no war at all.

Now suppose that the theory predicts correctly 99.5 percent of the time—meaning, of course, that it is a spectacularly good social science theory. In this period there will then be 9,950 instances where rational deterrence saved two nations from war. But there will also be 50 wars or crises. If the analyst chooses to write solely about the latter, the book that results will be full of nothing but deterrence failures. In short, *studies of crises and wars give no information about the success rate of rational deterrence*.

This problem has been recognized in the case-study literature, but its devastating implications have not been appreciated. Jervis recognizes the "error,"[54] as do George and Smoke;[55] but, in the absence of a solution to the problem, both continue with their analysis and the attendant pitfalls. The problem is most severe with Lebow who studies only "acute" crises, defined (retrospectively by him) as posing "a significant prospect of

[54] Jervis, "Perceiving and Coping with Threat" (fn. 1), 13.
[55] George and Smoke (fn. 1), 516-17.

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war."[56] Needless to say, this further selection criterion compounds the bias and makes it even more difficult to interpret any "generalizations" that might result.[57] Only Russett and Huth, focused on sampling issues by the quantitative tradition in which they work, are alert to its specific consequences for their conclusions.[58]

There is nothing wrong with nonrandom samples so long as they are not treated as random. Indeed, there may be good reasons not to choose cases randomly. Thus Mearsheimer, a defender of rational deterrence, studies a dozen major cases, of which ten (83 percent) are deterrence failures.[59] One cannot thereby conclude, however, as does Huntington, that there exists an "83.3 percent failure rate for rational deterrence."[60] The failure rate represents the author's choice of cases, not a feature of the international system; and the 83 percent figure would hold no matter how often rational deterrence worked in the larger universe of deterrence situations.

Case studies of deterrence typically use an informal research design in which cases are chosen at least partially by their score on the dependent variable (deterrence success or failure). When the cases are then misused to estimate the success rate of deterrence, the design induces a "selection bias" of the sort familiar from policy-evaluation research.[61] The seriousness of the difficulty is readily apparent once the research design is specified in the conventional sampling framework. Without that discipline, the bias is not easily detected.

How might a proper sample be constructed to assess the effectiveness of deterrence? The answer, of course, is by random sampling from the appropriate population. But how should the population be defined? This fundamental question, which has received no discussion in the case-study literature, is critical to any discussion of rational deterrence theory. No one has given a fully satisfactory answer.

[56] Lebow, *Between Peace and War* (fn. 1), 11; see also Snyder and Diesing (fn. 1), 6.

[57] An excellent example of the difficulties of understanding the implications of different selection criteria is provided by the case of the 1958 Quemoy crisis, which Russett (fn. 23) treats as a deterrence success, George and Smoke (fn. 1) treat as a deterrence failure, and Lebow, *Between Peace and War* (fn. 1), 13, excludes entirely because he judges it insufficiently "acute."

[58] Paul Huth and Bruce Russett, "What Makes Deterrence Work? Cases from 1900 to 1980," *World Politics* 36 (July 1984), 496-526. Levy (fn. 26) has provided a broad discussion of this issue for the quantitative deterrence literature.

[59] Mearsheimer (fn. 1).

[60] Samuel P. Huntington, "Conventional Deterrence and Conventional Balance in Europe," *International Security* 8 (Winter 1984), 38.

[61] James Heckman, "Sample Selection Bias as a Specification Error," *Econometrica* 47 (January 1979), 153-61; G. S. Maddala, *Limited-Dependent and Qualitative Variables in Econometrics* (Cambridge: Cambridge University Press, 1983); Christopher H. Achen, *The Statistical Analysis of Quasi-Experiments* (Berkeley: University of California Press, 1986).

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Presumably the relevant population for any time period consists of those states that have a "serious" dispute with another state, where "serious" is defined by the potential for war. There is no difficulty with this definition when deterrence fails and violence ensues: the dispute is *seen* to be serious. But what if no war breaks out? Have the Soviets stayed their hand from Western Europe because of NATO or because of satisfaction with the status quo? Pacific intentions are easily confused with successful deterrence, and vice versa.

The relevant population for deterrence, then, is not easily specified. One imperfect but useful approach is to employ a proxy, such as common borders, for the unobservable hostile intentions. Historical intuition would suggest that states with common borders are more likely to fight than states that do not have common borders.[62] The sampling population is then defined to be the set of all contiguous pairs of states. Creating a sample of all such pairs will include some irrelevant peaceful dyads with no disputes, but it will also include many relevant hostile pairs. When a number of such proxies are employed, the result is a sample that includes as a subset most of the relevant cases.

Procedures like these are frequently used in the quantitative literature to generate a suitable sample. The frequency of war is then computed for various types of deterrence relationships, as in Ferris and in Weede.[63] Both scholars find that rational deterrence has played a substantial part in preventing war. Thus, different conclusions about the effectiveness of deterrence emerge when samples are drawn scientifically rather than informally. Moreover, the formal procedures make clear where the remaining imperfections of these studies lie and how they might be reduced.[64]

Rational Deterrence and Decision Makers' Calculations

There is a second objection to the empirical testing of rational deterrence in comparative case studies—namely, that the verification proce-

[62] Harvey Starr and Benjamin A. Most, "A Return Journey: Richardson, 'Frontiers' and Wars in the 1946-1965 Era," *Journal of Conflict Resolution* 22 (September 1978), 441-67.

[63] Wayne Ferris, *The Power Capabilities of Nation-States* (Lexington, MA: Lexington Books, 1973); Erich Weede, "Extended Deterrence by Superpower Alliance," *Journal of Conflict Resolution* 27 (June 1983), 231-54.

[64] Study designs like those of Ferris and Weede lack textbook purity, but the clarity of their procedures makes it possible to assess the direction of remaining biases unambiguously. It is clear that both estimate the effect of rational deterrence conservatively. That is, the clutter of irrelevant, nonconflictual dyads in the samples underestimates the effectiveness of deterrence. For example, suppose, with Weede, that alliance with the same superpower is estimated to reduce the dyadic probability of war by 10%. Then suppose that half the nation-pairs in the sample are not really relevant—that is, they never had hostile intentions toward each other. Then the 10% difference derives only from the relevant half of the sample, making the true difference twice as large, namely 20%.

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dure fails to engage the rational deterrence theory of how states behave. In particular, rational deterrence is implicitly misconstrued as a theory of how decision makers think.

Case study analysts have often expressed the opinion that, if decision makers do not really carry out the appropriate mental calculations, rational deterrence theory does not apply. This argument is the "descriptivist fallacy." As a glance at any appropriate text will show, the axioms and conclusions of utility theory refer only to choices. Mental calculations are never mentioned: the theory makes no reference to them. Indeed, a major reason for the various axiomatizations of expected-utility theory is to show that decision makers need not calculate. If they simply respond to incentives in certain natural ways, their behavior will be describable by utility functions.[65] Rapoport puts it this way:

> Now, in being asked to choose between the two [lottery] tickets, the man is not asked to calculate anything. He is asked simply to choose between the two. It is from his *choices* that the theoretician will construct the man's utility scale on which all the outcomes will be assigned numbers (utilities).[66]

To our knowledge, no one who does rational-choice theorizing disputes this point, but it seems not to be widely understood.

Rational deterrence theory does contain some minimal psychological content: for example, the initiator must realize that the defender exists and threatens to defend. But rational deterrence is agnostic about the actual calculations decision makers undertake. It holds that they will act as if they solved certain mathematical problems, whether or not they actually solve them. Just as Steffi Graf plays tennis as if she did rapid computations in Newtonian physics (and in game theory, too—at least against Navratilova), so rational deterrence theory predicts that decision makers will decide whether to go to war as if they did expected-utility calculations. But they need not actually perform them.[67]

To avoid misunderstandings, we want to point out that our point here has nothing to do with Friedman's famous argument that the truth of assumptions need not matter.[68] Friedman maintained that a theory might

[65] R. Duncan Luce and Howard Raiffa, *Games and Decisions* (New York: John Wiley, 1957), 31-32.

[66] Anatol Rapoport, *Two-Person Game Theory: The Essential Ideas* (Ann Arbor: University of Michigan Press, 1966), 34.

[67] We are ourselves divided over whether international decision makers often do carry out expected utility computations to a reasonable approximation, or whether rational choice theory will usually be hopelessly inept at describing their conscious mental processes. Our point is that either view is consistent with the theory so long as their *behavior* follows rationality axioms.

[68] Milton Friedman, "The Methodology of Positive Economics," in *Essays in Positive Economics* (Chicago: University of Chicago Press, 1953).

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sometimes work well for certain purposes, even though its assumptions were known to be *false*. We have our doubts; but whatever its merits, this line of reasoning has no bearing on our argument. Our point is that even if decision makers do not actually calculate, or if they rationalize their actions after the fact with foolish calculations, the assumptions of rational choice theory may yet remain *true*.

Understanding what rational deterrence demands of decision makers is important because it sharply limits what counts against the theory. A proper understanding of the theory eliminates most of the arguments in the historical deterrence literature that are supposed to overturn it. Consider, for example, the fact that many decision makers do not seem to think in terms of probabilities at all. This point is made by Steinbruner with respect to Kennedy's remark that he faced certain impeachment if he did nothing about the Cuban missiles;[69] Stein has made it in connection with Anwar Sadat's forecasts of various foreign policy consequences for Egypt in the early 1970s.[70] Both authors maintain that, because each decision maker failed to discuss probabilities in situations where rational deterrence theory implies he should, rational deterrence is inaccurate in describing what happened. Related arguments have been advanced by Jervis and by Betts.[71]

These conclusions do not follow. Let us repeat that rational deterrence theory deals with choices, not mental calculations. It makes no predictions about what decision makers *say* influenced them, only about what actually did so. The distinction is particularly important in the case of postdecision reconstructions. As diplomatic historians have long been aware, the historical record often differs sharply from decision makers' memories, even memories about their own thoughts at the time.[72] Since the critics of deterrence wish to adduce evidence that is not only dubious but apparently irrelevant to the theory under discussion, they bear the burden of proof. They must show that no plausible psychological mechanisms could have introduced a gap between the reality and the reconstruction, and they must demonstrate that what decision makers say influenced them is identical with what actually happened.

Neither Stein nor Steinbruner have made such a case. Indeed, several plausible psychological mechanisms would render their evidence irrelevant—mechanisms of the sort that they and others have often put forward as key factors in international decision making. Suppose, then, that

[69] Steinbruner (fn. 2), 110.
[70] Stein, "A View From Cairo" (fn. 1), 55.
[71] Jervis, *Perception and Misperception* (fn. 1); Betts (fn. 1).
[72] For some striking examples, see *ibid.*, 115-16, 121-25.

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decision makers actually behave according to rational-choice theory. Indeed, no matter what he said, it is hard to believe that Kennedy's Cuban-missile decisions were unaffected by the difference between a *possibility* of impeachment, a *likelihood* of impeachment, and a *certainty* of impeachment. But suppose that, like the rest of us, Kennedy subsequently bolstered his decision by thinking and saying that any other choice would have brought on unpleasant political consequences with certainty. If so, he would speak in the apparently nonrational language that he actually used, but his behavior would be predicted perfectly by rationality assumptions.

Obvious competing interpretations like this one have not been excluded by the critics of rational deterrence. It is not easy to do so. Indeed, no matter how detailed the historical records, disagreements break out routinely over interpretations of decision makers' thoughts and intentions.[73] The degree of uncertainty in historical interpretations of motive and intention is often substantial, and conclusions deriving from such interpretations must be discounted appropriately. Yet case-study analysts usually provide no assessment of the reliability of their historical judgments. In particular, without a detailed showing that the questionable memories of decision makers represent the historical process accurately, no amount of evidence about their apparent calculation errors is relevant to the issue of whether rational deterrence theory predicts decision making.[74] In the absence of such a showing, rational deterrence theory stands unrebutted.[75]

It is not our central point here to set out one particular list of inferential

[73] See, for example, John Orme's critique of Lebow's historical research in "Deterrence Failures: A Second Look," *International Security* 11 (Spring 1987), 96-124.

[74] The relative unreliability of historical interpretation helps to explain why the quantitative literature on deterrence so often concentrates on power ratios and alliance bonds as causes of deterrence failure while ignoring mental events. The econometric theory of errors in variables demonstrated long ago that, to avoid serious inferential blunders, noisy data like self-reports of international decision makers must be discounted well beyond what common sense would suggest. This point is much less obvious within the informal inferential norms of the case-study tradition.

This does not imply that decision makers' self-reports need be problematic in all contexts. For example, survey research reports of preferences for political candidates will not ordinarily be subject to strong distortions, and they may therefore be used in tests of rational-choice assumptions. In general, tests of rational-choice theory based on reported preferences rather than on observed actions are tests of the joint proposition that the theory is correct and the reports are accurate. Only when we can have confidence in the latter proposition does the evidence bear on the rational-choice theory under consideration. (We are indebted to Henry Brady for this latter point.)

[75] The attentive reader will note that "unrebutted" does not mean "supported." In particular, to read our hypothetical reconstruction of Kennedy's thinking as an attempt to provide evidence *for* rational deterrence theory is to miss our point. What we are trying to demonstrate is that all such arguments are inherently flimsy in the absence of other evidence, and that such evidence is usually missing in the case-study literature, just as it is in our reconstruction.

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slips by case-study analysts. After all, one can imagine case studies that select cases in a nonprejudicial fashion, and that take proper account of the fallibility in assessments of decision makers' intentions. Well-designed case-study tests may not be decisive, but they can be highly enlightening and strongly persuasive.[76] They are certainly an indispensable first step before proceeding to statistical methods—a point quantitative researchers have too often ignored at their peril.

In principle, case studies are capable of doing everything statistical analysis can do; they may simply replace symbols with words and replicate the random samples, precise definitions, and rigorous inferences of statistical methods. But in practice, inferential rigor is not the best tool for what case studies should accomplish, and the comparative method works best when it enforces no such discipline. As Jack Snyder notes, "The best analysts in the [case study] field have always used a rough-and-ready version of the scientific method," which they have identified, not with the powerful counterintuitive logic of the real McCoy, but rather with the substantially tamer "self-conscious, systematic application of common-sense rules of inference."[77]

When the purposes of comparative case studies are properly understood, informality is the right choice. Creativity is enhanced when historical cases can be chosen at liberty and analyzed in accord with the investigator's intuitions. The great many degrees of freedom in these studies are inordinately helpful in finding useful variables and producing empirical generalizations. Like all good things, however, this free play for unaided common sense has a price. Used in isolation from scientific methods, it creates inevitable inferential errors and makes decisive theory verification well-nigh impossible. In case studies of rational deterrence, the consequences have been both predictable and devastating for the credibility of their conclusions.

Conclusion

Although many of our comments have criticized how case studies are used in practice, we emphatically believe that they are essential to the development and testing of social-science theory. They have the same claim to scientific status as formal theory, statistical methods, or any other research tool.[78] In international relations, only case studies provide the in-

[76] For example, Mearsheimer (fn. 1); Posen (fn. 1); Walt (fn. 1).

[77] Jack L. Snyder, "Richness, Rigor, and Relevance in the Study of Soviet Foreign Policy," *International Security* 9 (Winter 1984-85), 89-108.

[78] George and McKeown (fn. 9), 54.

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tensive empirical analysis that can find previously unnoticed causal factors and historical patterns such as those discussed by George and Smoke. But empirical generalizations and lists of variables are just a first step. They can substitute neither for theorizing nor for empirical verification. To misuse case studies in an effort to get what they cannot provide slights both theory development and empirical investigation.

Analysts who employ case studies of deterrence have done well at producing lists of variables that influence deterrence success, along with certain empirical generalizations about how different types of crises unfold. But they have produced no impressive general propositions to compare with those of rational deterrence.

Case-study generalizations are not a substitute for theorizing; empirical laws should not be mistaken for theoretical propositions. More than anything else, the hallmark of good theory is a fecundity that logically entails novel, sometimes surprising, insights and predictions. Inductive "theory" lacks this fecundity because it contains too few logical constraints. Categories can be multiplied to fit all cases. "Surprises" emerge not from the generalization, but from the case. Hence, we often cannot tell a consequential finding from an artifact; and when we succeed, the next case makes us begin all over again. By contrast, deductive theoretical propositions are of interest precisely because they interconnect with one another and prevent arbitrary multiplication of explanatory categories. The investigator is forced to be coherent.

Multiple case studies are also no substitute for statistical testing of theoretical propositions; contingent empirical generalizations should not be confused with confirmed statistical regularities. Here again, the informal procedures of case-study analysis provide too few constraints on the imagination of the analyst. In consequence, empirical evidence from case studies is rarely strong enough to falsify a theory. In the case-study literature on deterrence, these weaknesses have appeared, first, in selection bias—the tendency to oversample deterrence failures—and, second, in the uncritical use of decision makers' own reconstructions of their thinking. Each of these virtually guarantees a negative evaluation of rational deterrence theory, no matter how well it actually performs. Only statistical analysis, with its formal criteria for inference, is likely in practice to provide honest tests of theories.

Case studies are an important complement to both theory-building and statistical investigations, for precisely the reason Russett and George indicate: they allow a close examination of historical sequences in the search for causal processes.[79] The analyst is able to identify plausible causal var-

[79] Russett (fn. 53); George (fn. 6).

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iables, a task essential to theory construction and testing. Comparison of historical cases to theoretical predictions provides a sense of whether the theoretical story is compelling, and yields indispensable prior knowledge for more formal tests of explanatory adequacy. The method also generates novel empirical generalizations, which pose puzzles and challenges for theory to explain. In all these ways, case studies provide guidance in the revision and reformulation of analytic theory to account for a broader range of phenomena. Indeed, analytic theory cannot do without case studies. Because they are simultaneously sensitive to data and theory, case studies are more useful for these purposes than any other methodological tool. Too often, however, their findings have been interpreted as bodies of theory and tests of explanatory power. It is to these misuses that our criticisms have been directed.

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DETERRENCE AND FOREIGN POLICY

By ALEXANDER L. GEORGE and RICHARD SMOKE*

THE article by Christopher Achen and Duncan Snidal in this issue, however provocative some of its observations, has the merit of raising an important question: How should we think about the relationship between deductive theories of deterrence on the one hand and, on the other, research on deterrence that is at the same time empirical and oriented toward theory? While forcefully defending the deductive form of theory, Achen and Snidal also clearly dissociate themselves from the extreme position that it is a self-contained enterprise that need not take empirical research seriously. Rather, they recognize that empirical case studies are capable of contributing, and, indeed, have to some extent already contributed, to the development of theory, perhaps even to the kind of deductive theory they favor.[1]

Their general position on the need for both approaches—although not the superiority and priority they attach to deductive deterrence theory—is entirely compatible with our own long-standing position. The question to be debated is not the superiority of one approach over the other but rather how to improve both approaches and develop fruitful interaction between them.[2] On this score, the Achen-Snidal article is disappointingly

* For helpful comments on an earlier draft the authors express appreciation to Kenneth A. Oye, Jack S. Levy, and Robert Jervis who, however, bear no responsibility for the thoughts expressed in this comment.

[1] Thus: "in the hands of George and Smoke, the case study approach helps generate theory in a very direct way" (p. 156); "we emphatically believe that they [case studies] are essential to the development and testing of social science theory" (p. 167); "indeed, analytic theory cannot do without case studies" (p. 169; see also p. 159). Achen and Snidal, "Rational Deterrence Theory and Comparative Case Studies," *World Politics* 41 (January 1989), 143-69.

[2] Alexander L. George and Richard Smoke, *Deterrence in American Foreign Policy: Theory and Practice* (New York: Columbia University Press, 1974). Since publishing this book, we have worked steadily to raise standards and explicate procedures for improving the quality of case studies and their contribution to theory development. We have given serious attention to most of the concerns about them raised by Achen and Snidal. See A. L. George, "Case Studies and Theory Development: The Method of Structured, Focused Comparison," in Paul Gordon Lauren, ed., *Diplomacy: New Approaches in History, Theory and Policy* (New York: Free Press, 1979); A. L. George, "Case Studies and Theory Development," paper presented to the Second Annual Symposium on Information Processing in Organizations, Carnegie-Mellon University, Pittsburgh, PA, October 15-16, 1982; A. L. George and T. J. McKeown, "Case Studies and Theories of Organizational Decision Making," in Robert Coulam and Richard Smith, eds., *Advances in Information Processing in Organizations*, Vol. 2 (Greenwich, CT: JAI

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meager in its suggestions, in part because it concentrates on real or alleged limitations of case-study research and has little to say about how the deductive theory of deterrence, which they admit is deficient, might be improved.

In reporting the results of our empirical research on deterrence fifteen years ago, we called attention to the limitations of the deductive, abstract version of the theory then extant; we did not reject the approach itself. In fact, as Achen and Snidal come close to recognizing (fn. 41, and even more so in an earlier version of their paper), the reformulation of deterrence theory we advanced in Chapter 17 of our book presented a number of more refined propositions that suggested how deductive theory might be improved. This was, to be sure, incidental to our major objective, which was to demonstrate by means of a relatively new research strategy ("structured, focused comparison"[3]) how a relatively small number of historical case studies could contribute to developing a more differentiated theory than deductive theory offered, one composed of conditional generalizations (as exemplified in Chapter 18) that had more specific relevance for policy making.[4] In this research strategy, individual cases are used as building blocks to create a cumulative development of typological explanatory theory. Since the cases selected for study in this approach do not constitute a representative sample of the entire universe (a goal that is probably infeasible in any case), typological theory does not provide a basis for estimating the frequency distribution of different outcomes and, contrary to Achen and Snidal (p. 160), we have explicitly denied any such claim for it.[5]

Press, 1985), 21-58. Case-study methodology is discussed also in Richard Smoke, *War: Controlling Escalation* (Cambridge: Harvard University Press, 1977), chap. 3 and app. B. George continues efforts to improve case-study methodology and plans additional publications.

A discussion of the uses and limitations of the controlled comparison method of studying a small number of cases for theory development should not overlook the importance of single-case studies. Lack of space prevents us from summarizing the arguments on behalf of the contributions a single case study can make to theory development that have been advanced by such writers as Harry Eckstein, "Case Study and Theory in Political Science," in F. I. Greenstein and N. W. Polsby, eds., *Handbook of Political Science*, Vol. 7 (Reading, MA: Addison-Wesley, 1975), 79-138, and Lawrence B. Mohr, "The Reliability of the Case Study As a Source of Information," in Coulam and Smith, *op. cit.*, 65-97.

[3] The description of this method in George and Smoke (fn. 2), 95-103, has since been elaborated, and related methodological and theoretical issues more fully discussed, in the two articles by George (fn. 2) and in the George-McKeown article (fn. 2).

[4] The formulation of contingent generalizations is necessary in order to capture the fact that deterrence is characterized by the phenomenon of what General Systems Theory refers to as "equifinality" (and what John Stuart Mill referred to as "plurality of causes" when he warned that his "method of agreement" and "method of difference" could not be easily employed for inferring causal relationships). Equifinality refers to the fact that similar outcomes on a dependent variable (e.g., deterrence failures) occur as a result of different causal processes, thus making the search for robust universal causal generalizations infeasible. Abstract deductive deterrence theory has thus far ignored the phenomenon of equifinality.

[5] See George (fn. 2, 1979), 59-60.

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In an additional departure from existing deterrence theory, we disaggregated it into several of its components, which we called "initiation theory," "commitment theory," and "warning and response" theory (Chapters 17, 19, and 20).[6] We did so because we believe that an internally more complex form of deterrence theory will be necessary in order to enhance its policy usefulness. Thus, for example, we need to know more than rational deterrence theory tells us before we can better understand under what conditions, why, and how a state dissatisfied with the status quo will choose to challenge deterrence. Needed for this purpose is an "initiation theory" that is much richer and more complex than that provided by the deductive theory of deterrence.

In another departure from abstract deductive deterrence theory, we found it necessary to reconceptualize the problem of deterrence for different levels of conflict. These are (1) the deterrent relationship of the two superpowers' strategic forces; (2) the deterrence of local and limited wars; and (3) the deterrence of nonmilitary challenges and "sublimited" conflict at the lower level of the spectrum of violence. We noted that the first of these three levels had received the greatest and most successful attention in the formulation of deterrence theory. Largely because deterrence theory at the strategic level, dealing as it does with a relatively simple structural situation, was so much better developed, theorists were tempted to employ the logic of strategic deterrence as the paradigm case for thinking about deterrence in general. This has proven to be quite unsatisfactory, however, for there are major differences in the problem of applying deterrence strategy effectively at the second and third levels of conflict. Deterrence at those levels is much more dependent upon context than at the strategic level (although contextual variables also affect strategic deterrence—witness the concern over conditions and events that can contribute to crisis instability and inadvertent or accidental war). Thus, situations of substrategic conflict, in which the policy question of whether and how to employ deterrence arises, are often very fluid, ambiguous, and subject to unexpected and substantial changes. The interests and motivations (and hence the objectives) of one or both sides are often much more complex and unstable than in the simpler, paradigmatic strategic case.[7] And, finally, it is also more difficult to determine the means that

[6] This kind of disaggregated theory is an example of what Paul Diesing, drawing upon Abraham Kaplan, has referred to as "concatenated theory" in his *Patterns of Discovery in the Social Sciences* (Chicago: Aldine-Atherton, 1971), 22-24.

[7] Particular attention must be given not only to the strength of the Initiator's motivation to challenge the status quo but also to the time he has in which to do so. Moreover, asymmetry of motivation favoring the Initiator can sometimes compensate for asymmetry of power favoring the Defender. On this point see, for example, A. L. George et al., *The Limits of Coercive*

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are likely to provide effective deterrence, i.e., the type and level of potential damage considered necessary and adequate to convey a credible and sufficiently potent threat.[8] As a result, not only are the requirements for deterrence often more complicated and elusive for those levels of conflict; they are also more difficult both to identify reliably and to meet.[9] The problems of deterrence at different levels of conflict offer an additional challenge to deductive deterrence theory that it has not yet addressed and that are not recognized in the Achen-Snidal article.

We need to comment also on the inadequacy of rational deterrence theory's current formulation of the utility framework for dealing with the cost-benefit calculations of a state contemplating whether to challenge deterrence. The theory's assumption that a would-be initiator decides whether to "attack" or "not to attack" is oversimplified and inadequate for prediction. As we noted in our book,[10] the Initiator's choice is often not so limited; rather, the Initiator often has multiple options at his disposal for challenging a deterrence commitment, and his cost-benefit calculations are geared to identifying an option that offers an opportunity for gain while minimizing the risk of an unwanted response by the Defender. A more complex model of strategic interaction than Achen and Snidal offer is needed to grasp the interplay between a Defender who employs deterrence strategy and an Initiator who is considering not merely *whether* to challenge but *how best* do so at an acceptable cost-benefit level. Employing such a model of strategic interaction enabled us to score some cases as having mixed outcomes, i.e., the deterrence strategy employed may have succeeded in dissuading the Initiator from choosing some riskier options for challenging the status quo but it failed to dissuade the Initiator from employing "limited probes" or "controlled pressure" strategies to bring about change. This conceptualization of the interaction process also enabled us to identify several different causal patterns of deterrence failure (the phenomenon of equifinality we referred to in foot-

Diplomacy (Boston: Little, Brown, 1971); Zeev Maoz, "Resolve, Capabilities, and the Outcomes of Interstate Disputes, 1816-1976," *Journal of Conflict Resolution* 27 (June 1983), 195-229; Jack S. Levy, "When Do Deterrent Threats Work?" *British Journal of Political Science* 18 (October 1988), 433-60.

[8] Robert Wilson notes the difficulty that game-theoretic models encounter with contextual variables, which render most situations indeterminate; "Deterrence in Oligopolistic Competition," in Paul C. Stern et al., eds., *Perspectives on Deterrence* (New York: Oxford University Press, forthcoming).

[9] Examples of difficulties encountered in attempts to apply deterrence strategy at substrategic levels of conflict are provided in the analysis of the Eisenhower administration's Middle East policies in George and Smoke (fn. 2), chap. 11. For a discussion of thirteen ways in which the characteristics of deterrence at the strategic level differ from those at lower levels of conflict, see chap. 2.

[10] *Ibid.*, 520-22.

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note 4) which, as Achen and Snidal acknowledge (p. 156), their rational deterrence theory has not considered and which they regard as of major importance for theory as well as policy.[11]

The preceding remarks have raised the problem relating to levels of analysis in constructing deterrence theory and, even more, in utilizing it in policy-making contexts. Achen and Snidal evidently prefer to hold fast to a deductive theory, one that "Black Boxes" both the decision-making and strategic-interaction levels of analysis. It is not easy for the reader to grasp how they reconcile this preference with their recognition of the value of findings from empirical research that attempt to study actual events in these two Black Boxes rather than simply make assumptions about decision-making and strategic-interaction processes. Left unanswered in their essay is whether and how they would integrate these empirical findings into a new deductive deterrence theory or how they would make the connection between the two types of theory. In other words, how do they propose to deal with the deterrence phenomenon at different levels of analysis?

This leads us to raise a number of questions regarding the nature of the deductive rational deterrence theory espoused by Achen and Snidal and to pose the critical question of how it might be improved and still remain a deductive theory. Although the major impetus and thrust of their article is to defend deductive deterrence theory, Achen and Snidal clearly admit the absence of a satisfactory rational deterrence theory. They acknowledge that coherent supporting arguments for such a theory are lacking, and state that what they are really praising are its foreseeable conclusions when it is properly developed.[12] Nonetheless, they vigorously praise the contributions it has already made, while at the same time arguing merely that rational deterrence theory should not be abandoned for what they dismiss as "the patchwork of empirical findings currently available" (p. 159). And, toward the end of the essay, they further dilute their defense of this particular theory by concluding: "More importantly, our goal here is to defend theory, not rational deterrence theory" (p. 159).

While the forthright admission that rational deterrence theory remains undeveloped is praiseworthy, the authors should at least tell us what they think an axiomatic-deductive theory must be like in order to be capable

[11] *Ibid.*, chap. 18.

[12] See particularly fn. 24 in Achen and Snidal (fn. 1): "Analysts continue to struggle painfully for a fully satisfactory version of this game; as Harrison Wagner has remarked to us, 'the rational theory of deterrence' doesn't exist." However, they go on to offer the confident assurance that "the principal conclusions of a legitimate theory of deterrence are foreseeable even if the supporting arguments are at present incomplete; it is the former that we call 'rational deterrence theory.'"

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of predicting the success or failure of deterrence in specific cases. That it is not currently a full-fledged deductive theory is evident, but what is missing? Have the requirements and standards for such theories been clearly and fully identified and agreed upon by proponents of deductive theory? Do Achen and Snidal adhere to the same conception of these requirements as Harrison Wagner or Bruce Bueno de Mesquita; or are they satisfied with the operationalization of deterrence provided by Huth and Russett.[13]

The task of developing a full-fledged deductive deterrence theory may be much more demanding than Achen and Snidal anticipate. Even if the internal logical structure of such a theory is eventually fully developed, it remains to be seen how well it will perform in making predictions of deterrence outcomes in specific cases. Achen and Snidal offer no clear conception of what kind of axiomatic-deductive theory would be capable of making such predictions. To achieve this capability, rational deterrence theory requires not merely internal logical consistency; it must also be *operationalized* so that the presence or absence of the conditions assumed to be necessary and/or sufficient for deterrence to be effective can be measured and established in each of the specific cases for which predictions are attempted. Since the theory is not yet operationalized, it provides no basis for predicting whether a specific deterrence threat will be sufficiently credible and potent in any particular case. Lack of operationalization leaves rational deterrence theory weakest precisely where it needs to be strongest if it is to be able to make predictions in specific cases, i.e., in assessing the utility calculations of the Initiator in a particular situation. (It was for this reason that fifteen years ago we emphasized the neglected importance of "Initiation Theory" in the abstract deductive theory of that era.[14])

We shall return to problems of operationalizing deductive deterrence theory but, first, we would like to make several observations about its assumption of rationality. An assumption of some kind of "rationality" in deductive deterrence theory is necessary and useful as a starting point, but a general, unrefined assumption of rationality appears to us as grossly insufficient to operationalize the theory adequately. We do not reject the usefulness of the general assumption of rationality—even "pure rationality"—for the development of theory. But, as Thomas Schelling cautioned many years ago, "whether the resulting theory provides good or poor insight into actual behavior is ... a matter for subsequent judg-

[13] Paul Huth and Bruce Russett, "What Makes Deterrence Work? Cases From 1900-1980," *World Politics* 36 (July 1984), 496-526.

[14] George and Smoke (fn. 2), chap. 17.

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ment."[15] Even if one operates with the additional assumption of a "unitary actor," a more specific version of the type of rationality assumed to be operating is required, if only to indicate how one believes actors in general, or the specific actor in question who is contemplating a challenge to deterrence, will deal with the well-known cognitive limits on rationality. (Not all actors deal with these cognitive limits in the same way.)[16] Herbert Simon's concept of "bounded rationality" is of some use but, as he recognizes, it is often insufficient for fine-tuning rational choice theories.[17] If the theorist wishes to continue to "Black Box" the decision-making process by means of assumptions (instead of getting into the Black Box to observe and/or estimate what is actually going on, *as policy makers with the assistance of their intelligence specialists attempt to do*), then it is often desirable to develop what Simon calls "auxiliary assumptions" as to the type of rationality that actors in general, or the particular actors about whom predictions are to be made, typically exhibit in situations of this kind.

Such auxiliary assumptions regarding rationality are often added to their models, as Simon notes, by investigators who formulate and employ deductive theory. But where do they come from? Not from contemplating one's navel but rather from reflecting on relevant empirical observations of actual behavior and converting such observations into auxiliary assumptions to be added to the theory and incorporated into its internal logical structure.

Important questions arise in addressing the task of operationalization, the answers to which are not self-evident. First, which of the key concepts of rational deterrence theory should be singled out for this treatment? Achen and Snidal, like other deterrence theorists, emphasize the importance of (1) the credibility of a deterrence commitment/threat, and (2) the need for a threat of punishment that is sufficiently strong to overcome the Initiator's inclination to challenge deterrence by influencing his cost-benefit calculations. Are these the key concepts of the theory to be operationalized? Or will deductive theorists avoid dealing with these particular concepts and settle for operationalizing the Initiator's utility calculations? This would require the investigator to determine whether, in contemplating a possible challenge to the status quo, the would-be Initiator ex-

[15] Schelling, *Strategy of Conflict* (Cambridge: Harvard University Press, 1960), 4. For a fuller statement of our concern over the limitations of the rationality assumption in deterrence theory see George and Smoke (fn. 2), 73-77.

[16] See, for example, the discussion of different ways in which individuals attempt to cope with the cognitive limits on rationality (i.e., inadequate information, inadequate knowledge, and value complexity) in A. L. George, *Presidential Decisionmaking in Foreign Policy* (Boulder, CO: Westview Press, 1980), chap. 2.

[17] Simon, "Human Nature in Politics: The Dialogue of Psychology with Political Science," *American Political Science Review* 79 (June 1985), 293-304. See especially pp. 295 and 300.

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periences "positive utility" or "negative utility" in a particular deterrence situation. If so, how useful would such operationalization of the theory be for predicting deterrence success and failure?

Second, what kind of predictions would a full-fledged, adequately operationalized deductive deterrence theory be capable of making, and how useful would such predictions be? Would such a theory modestly claim (as does Bueno de Mesquita, who regards "positive utility" as a necessary condition for war initiation[18]) to have identified only a "necessary" condition for deterrence success, leaving unidentified the all-important other conditions? If, for example, either the credibility or the potency of a deterrent threat (or, more generally, "negative utility") is only a necessary but not sufficient condition for deterrence success, then such a theory does not predict deterrence success but merely observes that the stipulated necessary condition would be present *if* a deterrence success occurred. The predictive power of the theory and its utility would be further diminished because a so-called "necessary" condition, such as credibility of the deterrent threat, could be present (as in the Pearl Harbor case) even in cases of deterrence failure.

On the other hand, would an adequately operationalized deductive theory make the much more ambitious claim that it identified one or more sufficient conditions for deterrence success? If so, what are the candidates for a sufficient condition? Credibility of a deterrent commitment, we already know from historical cases, cannot be considered a sufficient condition for deterrence success. In the Pearl Harbor case, for instance, the Japanese accepted the U.S. deterrence commitment as credible but nonetheless attacked, in part because the United States was also subjecting them to strong coercive diplomacy. Again, is a threat of punishment potent enough to turn the Initiator's cost-benefit calculations against an "attack" a sufficient condition? Not by itself, quite obviously, since such a threat must also be credible. Perhaps one can say that a sufficiently potent threat (whatever that is) is at least a "necessary," though certainly not a sufficient, condition for deterrence success. Such a restricted claim is consistent with what we have already learned from history: while "massive retaliation" was an enormously potent threat, it often lacked enough credibility and relevance to deter some types of challenges to deterrence commitments made by the United States on behalf of its foreign policy interests.

Evidently some combination of these two conditions—credibility and potency of deterrence threat—is relevant for deterrence. Leaving aside the important and not easily answered question of their required mag-

[18] See Bruce Bueno de Mesquita, *The War Trap* (New Haven: Yale University Press, 1981).

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nitude in any particular situation, difficult questions for deductive theory remain. First, if both these conditions are held to be necessary and/or sufficient, how can they be operationalized? This task is most formidable. The alternative—to avoid dealing directly with them and to focus, instead, on the would-be Initiator's "positive" or "negative" utility—would leave us with a disappointing, almost trivial, theory that could claim to identify only a necessary condition for challenging deterrence and, hence, could not really predict deterrence outcomes.

A second issue would arise if, in applying such a theory, one encountered historical cases in which both conditions required by the theory were measured as being present but, nonetheless, deterrence failed. How would the deductive theory explain such anomalous results? Would the proponents of the deductive theory "save" it by arguing that the incorrect predictions must be due to measurement errors that could in principle be reduced or eliminated by improvements in its operationalization? Or would they concede the possibility that the deductive theory leaves out variables (e.g., domestic political constraints, psychological impediments to optimal information processing, misperception and miscalculation of the opponent's intentions) that affect deterrence outcomes, or that it makes defective assumptions about rationality and strategic interaction? How would deductive theorists decide which of these two possible explanations—measurement error or failure to consider other relevant variables—is correct?

Still another issue—the familiar and vexing one already encountered in empirical research on deterrence—concerns the possibility that the absence of a challenge to deterrence might be *spuriously* scored as a deterrence success. Thus, the conditions postulated by the theory as necessary and sufficient for deterrence success might be established as present in a particular case in which, to be sure, deterrence was not challenged, but nevertheless a claim that the theory has made a successful prediction might not be justified.

Alternative explanations for the absence of a challenge to the status quo, which are not encompassed by deductive deterrence theory, would also have to be considered. Thus, although dissatisfied with the status quo, the Initiator might not have contemplated challenging deterrence and, hence, need not have been deterred. Or, while considering a possible challenge, the Initiator may have decided not to attack for other reasons altogether (e.g., domestic or allied constraints, internal policy disagreements, the expectation of eventually securing a favorable resolution to his grievance by diplomacy), and not because of the presumed credibility and potency of the deterrence threat.

Finally, we feel obliged to say that the failure fully to specify and to

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operationalize rational deterrence theory encourages its defenders—unwittingly to be sure—to employ it in ways that risk giving the theory a tautological and untestable character. In its present non-operationalized form the theory is difficult, if not impossible, to refute; as a result, it is being endowed with a validity that is highly questionable, insufficiently qualified, and certainly premature. This is so because the theory, lacking operationalization, is not capable of generating predictions for specific cases. Instead, the outcomes of specific cases—once they are known—can all too easily be claimed to be consistent with the theory. After the fact, one can attribute historical cases of deterrence failure to lack of sufficient credibility and/or to an insufficiently potent threat of punishment. Similarly, ostensible cases of deterrence success can all too readily be chalked up to the use of a deterrence strategy that met the postulated requirements of the theory.

One way of adjusting to some of the difficulties we have noted would be to abandon the effort to state necessary and/or sufficient conditions and settle for the goal of constructing and validating a theory of deterrence that would at least be capable of making predictions on a probabilistic basis. A theory couched in probabilistic terms is not without value—if its expected performance can be specified and validated; but it should also invite efforts to identify additional conditions under which deterrence efforts are likely to succeed or fail. The kinds of "conditional generalizations" that can be abstracted from historical case studies of the kind we presented in our earlier work might be helpful in this respect.[19]

Certainly efforts to develop deductive deterrence theory should continue and be pressed forward. We have argued that the standards and requirements for such a theory and the type of predictions it would be capable of making should be explicated rather than obscured by general arguments for the superiority of this type of theory. If this is done, it will become obvious that the achievement of a robust deductive deterrence theory is far removed from, and in any case of quite uncertain value for, providing strong guidance for policy-making purposes. Neorealist structural theory in international relations, with which Achen and Snidal correctly connect rational deterrence theory, addresses only one level of analysis. The foremost expositors and proponents of neorealist structural theory explicitly concede that it is not a theory of foreign policy. Admittedly, a theory of foreign policy, strictly speaking, does not exist and, given the complexity and difficulty of the task, is not likely to emerge in robust form for many years, if ever. Neorealist structural theory, in our view, is necessary, but quite insufficient by itself either for adequately un-

[19] George and Smoke (fn. 2), chap. 18.

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derstanding and explaining much of importance in international relations or for managing foreign policy. Empirical studies of decision making and strategic interaction are necessary to fill, to some extent at least, the important vacuum it leaves.

Deductive deterrence theory in its present primitive, undeveloped form (which, unlike Achen and Snidal, we do not equate with the virtue of "parsimony") contributes very little to an understanding of the uses and limitations of deterrence strategy as an instrument of foreign policy. The possibility of unwarranted and dangerous uses of abstract deterrence theory as a basis for prescriptive advice to policy makers worried many in the early 1970s.[20] Ideas associated with the theory stimulated or accompanied the development of strategic theory, which, in turn, has provided a framework within which policy makers (not deductive theorists) have tried to address the operational requirements of strategic deterrence. Contrary to Achen and Snidal, however, we argue that whatever contribution abstract deterrence concepts made to the development of strategic theory, it was not matched by a similar contribution to an understanding of the uses and limitations of deterrence strategy at lower levels of conflict.

Whether deterrence theory is derived from deductive exercises or empirical research, we continue to argue—as we did fifteen years ago—for a sharper distinction between deterrence theory and deterrence strategy, and for a better understanding of the relationship between the two. Deterrence theory (limited and imperfect as it is) should be regarded as, at best, an aid to devising deterrence strategies. Its most relevant and responsible use for policy-making purposes is not its prescriptive power but rather its diagnostic function, i.e., its assistance to policy makers in assessing the configuration of a situation in which some kind of challenge to deterrence may occur. Since, as we argued and attempted to demonstrate in our book, deterrence at the substrategic levels of conflict is highly context-dependent, there is a critical need in policy making for good situational analysis.[21] This would assess the nature and strength of the Initiator's motivation, how urgently he feels the need to challenge deterrence, the options available to him for doing so, the kind of utility calculations and assessment of his options he is likely to be making, and which of them he is likely to choose, if any. Situational analysis of this

[20] See, e.g., *ibid.*, chap. 3.

[21] On situational analysis and the diagnostic function of theory, see George and Smoke (fn. 2), chap. 16; also George (fn. 16), chap. 14. For a more detailed discussion of policy-relevant theory see George and Smoke (fn. 2), app. A longer version of this appendix was published by Smoke and George, "Theory for Policy in International Relations," *Policy Sciences* 4 (December 1973), 387-413.

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kind is likely to provide a better understanding of the deterrence problem and of what variant of deterrence strategy (alone or together with other policy options) is likely to be most appropriate and effective in that particular situation. Since the context of even a single deterrence situation can and does change over time, situational analysis is a continuing task. Middle-range theory, derived through empirical analysis and expressed in the form of conditional generalizations, together with general models of the opponent's behavioral characteristics, are of some help in situational analysis, as are the findings of the more recent empirical studies of deterrence subjected to critical analysis by Achen and Snidal.

As these remarks indicate, the policy maker (unlike the deductive theorist) makes the attempt to get into the Black Boxes of decision making and strategic interaction. Would deductive theorists say the policy maker should not do so but, instead, merely rely on abstract rational deterrence theory? In our view deductive deterrence theory can give the policy maker very limited help in assessing the decision-making process of the opponent or in anticipating the outcome of the interaction between his own deterrence strategy and the adversary's choice of action. For this, the policy maker has to rely largely on the information and analysis provided by his intelligence and advisory system.

We have saved for the end of our comment three other major limitations of deterrence theory—whether deductive or empirical. The first is that the theory cannot define its own scope and relevance as a means of achieving foreign policy goals.[22] That is to say, as implied by the title given to our comment, deterrence theory per se provides no criteria to indicate when a deterrence policy should be applied in foreign policy. Often, but certainly not always (and particularly not at the substrategic levels of conflict), the criteria are self-evident or easily identified on an ad hoc basis. But, viewed in the broader context of foreign policy, as it should be, deterrence theory is best understood as a contingent policy theory: "*if* a state believes it necessary or desirable to employ deterrence in a particular situation, then the general requirements for devising an appropriate, hopefully viable deterrence strategy are the following . . ." Only a foreign policy theory, or a theory of statecraft, can provide guidance as to when a state should employ deterrence to protect specified types of interests as against (or in addition to) other strategies for protecting its interests and/or resolving points of conflict with other states. Deterrence strategy, in other words, must be viewed as only one of a number of different instruments of foreign policy. Failure to recognize this—either by theorists or

[22] See George and Smoke (fn. 2), 77-78.

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policy makers—can, and does, easily lead, as we argued in our book and continue to argue, to a narrow over-reliance on deterrence strategy in foreign policy.

Another, related limitation of deterrence theory is its exclusive preoccupation with threats of punishment as a means of persuading an adversary not to challenge a situation in which there is disagreement. Critics of deterrence, extending back to the early years of the Cold War, have emphasized (sometimes overemphasized) that threats can be counterproductive. We have argued that early deterrence theorists erred in their evident assumption that a viable theory of deterrence can be developed independently of a broader theory of processes by which nations influence each other, one that encompasses the utility of positive inducements as well as, or in lieu of, threats of negative sanctions.[23] Thus, for example, the credibility and potency of a deterrence threat might not be so critical if instead (or in addition) sufficient positive incentives for remaining at the status quo are offered.

Finally, deterrence theory ignores the frequent need to regard deterrence, even when it is necessary and effective, as a strategy to buy time. Deterrence can severely frustrate an adversary who is strongly motivated to change a status quo that he regards as invidious, especially when he feels it legitimate to do so. The consequences of continued frustration are not easily predictable and are not always favorable to the deterring power. Deterrence success in the short run is not always beneficial in the longer run; the adversary may become more desperate to mount a challenge and may proceed to acquire greater resources for doing so. Under such circumstances the most reliable benefit of successful deterrence may be more time—time which is best used not in a possibly futile effort to maintain deterrence indefinitely but to work out, if possible, an accommodation of conflicting interests so as to reduce reliance on deterrence and avoid overt conflict.[24]

[23] *Ibid.*, chap. 21.

[24] For additional discussion see George and Smoke (fn. 2), 5-6; also George (fn. 16), 252-54. The importance of "promises" (as against "threats") was briefly noted by Schelling (fn. 15), 43-46, 131-37, 175-77. Important leads for the development of a broader influence theory were given by David A. Baldwin, "The Power of Positive Sanctions," *World Politics* 24 (October 1971), 19-38, and "Inter-Nation Influence Revisited," *Journal of Conflict Resolution* 15 (December 1971), 471-86. The importance of conceptualizing the strategy of coercive diplomacy to include possible use of "carrots" as well as "sticks" was emphasized and illustrated in George et al. (fn. 7). The concept of "crisis bargaining" developed by Glenn Snyder and Paul Diesing includes accommodative as well as coercive actions; *Conflict Among Nations* (Princeton: Princeton University Press, 1977). Other writers, too numerous to mention here, have also emphasized the need for a broader, multifaceted theory of inducement or influence. More recently, Janice Gross Stein and Richard Ned Lebow have emphasized the need for more attention to various forms of "reassurance" to supplement or replace deterrence in conflict situations.

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RATIONAL DETERRENCE: Theory and Evidence

By ROBERT JERVIS*

AMONG the most important questions in social science are the causes and effects of threats and force.[1] In international politics we are particularly interested in when threats protect the state and when, by contrast, they set off a spiral of counterthreats that leave both sides worse off than they would have been had the state adopted an alternative policy.[2] Scholars and statesmen would find the problem easy if the effects were invariant, i.e., if a properly constructed policy of threats always protected the state, as it might have protected Britain and France in the 1930s, or if threats always proved ineffective or provocative, as may have been the case before World War I.[3] But in fact both threats and conciliation can produce the very consequences they were designed to avoid.

Of less concern to statesmen than to scholars are methodological debates over the relative advantages of deduction and induction[4] and the merits of theorizing based on the assumption that actors' behavior can be

* I am grateful to David Baldwin, Helen Milner, Kenneth Oye, Robert Shapiro, and Jack Snyder for comments and suggestions.

[1] For an assessment of the relevant evidence from outside international politics considered within the frameworks developed by studies of international deterrence, see Paul Stern et al., eds., *Perspectives on Deterrence* (New York: Oxford University Press, forthcoming).

[2] For an extended discussion, see Robert Jervis, *Perception and Misperception in International Politics* (Princeton: Princeton University Press, 1976), chap. 3. Also see Aaron Wildavsky, "Practical Consequences of the Theoretical Study of Defense Policy," *Public Administration Review* 25 (March 1965), 90-103. For a recent summary of the evidence, see Martin Patchen, *Resolving Disputes Between Nations: Coercion or Conciliation?* (Durham, NC: Duke University Press, 1988). A complicating factor is that many abstract and seemingly historical or scientific arguments are driven by political preferences because debates about deterrence are central to American foreign policy. Thus many disagreements about the origins of World Wars I and II or Khrushchev's rationality in putting missiles in Cuba are not purely academic.

[3] These cases are more complicated than the stereotypes commonly used by political scientists. See Robert Jervis, "War and Misperception," *Journal of Interdisciplinary History* 18 (Spring 1988), 685-88; Patrick Glynn, "The Sarajevo Fallacy," *National Interest*, No. 9 (Fall 1987), 3-32; Richard Ned Lebow and Janice Gross Stein, "Beyond Deterrence," *Journal of Social Issues* 43, No. 4 (1988), 33-35.

[4] See, for example, Oran R. Young, "Professor Russett: Industrious Tailor to a Naked Emperor," *World Politics* 21 (April 1969), 486-511; Bruce Russett, "The Young Science of International Politics," *World Politics* 22 (October 1969), 87-94; Klaus Knorr and James Rosenau, eds., *Contending Approaches to International Politics* (Princeton: Princeton University Press, 1969), esp. Marion Levy, Jr., " 'Does It Matter If He's Naked?' Bawled the Child."

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best interpreted as though they were seeking to maximize their subjective expected utilities (SEU).[5] In these debates, of course, much depends on whether a particular method is identified as one of several that are useful or as the only useful method. Furthermore, as Christopher Achen and Duncan Snidal note,[6] methods differ in their utility depending on the specific questions being asked and the part of the scientific process under consideration. Most obviously, without case studies we can have no evidence. Even if deductive models are parsimonious, rigorous, and produce fascinating propositions, they will remain little more than toys unless they can be verified.

When considering SEU, we must remember that the utilities and expected consequences of alternative policies are exogenous. Before SEU can tell us much, then, we have to tell it a great deal. We must, for instance, specify what situation the actor thinks he is facing, how he ranks his goals, what options he perceives, and how he thinks others are likely to react. Case studies have devoted much effort to determining the statesman's beliefs about his environment and adversaries and how and why he formed them, as well as to identifying common errors of procedure and substance.

The choice between the deductive approach and one that builds on case studies involves a trade-off between rigor and richness. A deductive theory must miss many facets of any individual case; the study of all the complexities of any specific example will fall short on generalization and perhaps on the confidence with which causal relationships can be established. In part, the choice is a matter of individual taste. But it may also reflect a judgment—or an intuition—about how regular the world is:

[5] Good reviews of these kinds of theories are Paul Schoemaker, "The Expected Utility Model: Its Variants, Purposes, Evidence and Limitations," *Journal of Economic Literature* 20 (June 1982), 529-63; and Mark Machina, "Choice Under Uncertainty: Problems Solved and Unsolved," *Economic Perspectives* 1 (Summer 1987), 121-54. Debates about this approach are not unique to political science. In anthropology the argument is joined, for example, by Marshall Sahlins, *Stone Age Economics* (New York: Aldine, 1972) and Marvin Harris, *Cultural Materialism: The Struggle for a Science of Culture* (New York: Random House, 1979). A recent summary of the arguments between psychologists and economists in the latter's own field can be found in the special issue of *The Journal of Business* 59 (October 1986). For the arguments in sociology, see Paul Hirsch, Stuart Michaels, and Ray Friedman, " 'Dirty Hands' Versus 'Clean Models': Is Sociology in Danger of Being Seduced by Economics?" *Theory and Society* 16 (May 1987), 317-36. Also, see Mark Gravovetter, "Economic Action and Social Structure: The Problem of Indebtedness," *American Journal of Sociology* 91 (November 1985), 481-510. Also see Brian Barry, *Sociologists, Economists, and Democracy* (Chicago: University of Chicago Press, 1978). For the purposes of this article, I will equate rationality with probability-utility calculus, as most authors do, but this glosses over the question of whether it is always rational to choose by this criterion. Doing so, for example, could mean following a policy that entailed some chance of enormous loss, and we might not want to say that a person who chose a safer policy with a lower expected utility was violating the tenets of all rational models.

[6] Achen and Snidal, "Rational Deterrence Theory and Comparative Case Studies," *World Politics* 41 (January 1989), 156-59.

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whether the phenomena we are most interested in more closely resemble clouds or clocks.[7] Similarly, employing SEU theories implies that there is a method in what seems like madness; those who reject this approach often have a very different sense of the patterns of human behavior.

It is often said that case studies are atheoretical in that they cannot generate new and powerful ideas. This gives them both too much and too little credit.[8] On the one hand, the implication is that the scholar looks at the evidence without any preconceptions and expectations that could distort her findings. But of course this cannot be: without previously formed ideas, no one could make any sense of the world. On the other hand, finding causal links in one case—or, to be more accurate, thinking one has found them—not only leads to the expectation that they may be operating in other cases, but can generate new propositions about, for example, the other conditions that might produce the same effect or the alternative consequences of the independent variables under different circumstances. The line between induction and deduction is often blurred; the understanding produced by case studies leads to conditional and probabilistic deductions about how statesmen are expected to behave.

These subjects are obviously too large and complex to be fully covered here, so my treatment will be selective. After making some distinctions, I will sketch the impediments to developing an expected utility model of deterrence and then turn to the use of case studies, first looking at the biases in case selection and then discussing what has been learned by studying deterrence failures. The central question here is whether these findings can be explained by a rational theory. Some of them can be, although such a theory would be quite different from our standard models of deterrence. Others are incompatible with even a reformulated SEU theory.

Debates and Distinctions

The debates mentioned in the previous paragraphs overlap only in part. First, deductive theorizing and expected utility models are not nec-

[7] See Gabriel Almond and Stephen Genco, "Clouds, Clocks, and the Study of Politics," *World Politics* 29 (July 1977), 489-522, and Albert Hirschman, "The Search for Paradigms as a Hindrance to Political Understanding," *World Politics* 22 (April 1970), 329-43.

[8] For good discussions, see Harry Eckstein, "Case Study and Theory in Political Science," in Fred Greenstein and Nelson Polsby, eds., *Handbook of Political Science*, Vol. 7 (Reading, MA: Addison-Wesley, 1975), 79-137; Alexander L. George and Timothy McKeown, "Case Studies and Theories of Organizational Decision-Making," in Robert Coulam and Richard Smith, eds., *Advances in Information Processing in Organizations*, Vol. 2 (Greenwich, CT: JAI Press, 1985), 21-58; Donald Campbell, " 'Degrees of Freedom' and the Case Study," *Comparative Political Studies* 8 (July 1975), 178-93.

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essarily the same. The latter can be applied to particular cases without attempting to generate new propositions. Indeed, much of history and political science consists of informal analysis along these lines. Scholars often seek to understand the goals, beliefs, and perceptions of decision makers that can make their policies intelligible, although applied to any single case the explanations must remain ad hoc and post hoc. Thus, much of the search for an explanation of why Khrushchev put missiles into Cuba has involved trying to discover whether he felt that there were great gains to be made (or losses to be avoided) by doing so, thus justifying considerable risk, or whether he believed that his actions were not particularly risky either because the United States would acquiesce or because he would be able to control any dangers that might arise.

The other side of this coin is that deductive theorizing need not assume rationality on the part of actors. Most theories in psychology are of this type. For example, cognitive dissonance, which posits that people seek to reduce psychological conflict after they have freely made an important decision, leads to many counterintuitive propositions. The attempts to model belief systems of people in general, people of a certain kind, or particular individuals also fit into the category of deductive, but nonrational, theories.[9]

Second, one should not equate expected utility arguments with the claim that all people will behave the same way in the same situation, that is, with arguments that ignore the individual level of analysis. If each person is rational, but has different values and means-ends beliefs, then behavior will be idiosyncratic. Thus expected utility theories are not necessarily parsimonious: to yield predictions or explanations they often require a great deal of information about particular individuals and situations. By contrast, prevalent irrationalities can produce strong and simple patterns.

Third, there is no simple relationship between methodological and substantive arguments. This is true in two senses. First, there is no inevitable connection between an SEU theory and one that stresses the use and efficacy of threats. Economics thrives on SEU models. Conversely, a theory of deterrence need not assume rationality. Indeed, it is quite possible that threats would have been challenged much more frequently were statesmen fully rational. The other's capability and credibility may be overestimated without warrant, in part because of the factors discussed below that under other circumstances can undermine deterrence. Furthermore,

[9] See, for example, the path-breaking study by Robert Abelson and Milton Rosenberg, "Symbolic Psycho-logic," *Behavioral Science* 3 (January 1958), 1-13. Artificial intelligence models are in this tradition.

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in some cases a small chance that an assertive policy could lead to war may deter, even though such caution is not easily squared with the rest of the state's values and behavior. Indeed, it is often argued—incorrectly, I believe[10]—that nuclear deterrence of attacks on a state's allies (known as "extended deterrence") is incompatible with full rationality. Second, although the use of case-study methodology may call rational theories into question, this is not always the case. Indeed, several of Alexander George and Richard Smoke's findings, particularly that deterrence can fail when challengers are able to design around threats, to use *faits accomplis*, or to exert controlled pressure, can be folded into a sufficiently complex rational theory.[11]

Finally, we must not equate failures of a deterrence policy with failures of deterrence theory.[12] Most obviously, the failure of a deterrent that was badly designed or implemented would not falsify the theory, although it would raise the question of why the state produced such a suboptimal policy. Furthermore, a state can rationally choose to fight a war it thinks it will probably lose if the gains of winning and/or the costs of alternative policies are great enough.[13] Similarly, deterrence successes do not confirm the theory if they occur under circumstances in which the theory indicates they should fail.[14] Parallel reasoning shows that successful policies are not necessarily the product of rational processes nor can they always be accounted for by rational theories; failures similarly do not prove irrationality. To take just the simplest case, wild gambles sometimes pay off, and policies that are optimal can prove disastrous in any particular case.

Rational Deterrence Theories

The first step in judging the validity of rational deterrence theory (RDT) is to establish whether it is intended to be descriptive, prescriptive, or both.[15] If prescriptive, it says what actors should do to maximize their

[10] See Robert Jervis, "Deterrence Theory Revisited," *World Politics* 31 (January 1979), 299-301, and *The Illogic of American Nuclear Strategy* (Ithaca, NY: Cornell University Press, 1984).

[11] Alexander L. George and Richard Smoke, *Deterrence in American Foreign Policy* (New York: Columbia University Press, 1974), 519-33. As Achen and Snidal (fn. 6) note, however, these propositions were reached inductively.

[12] For further discussion, see Achen and Snidal (fn. 6), 152; Richard Ned Lebow, "Deterrence: A Political and Psychological Critique," in Stern (fn. 1); George Quester, "Some Thoughts on 'Deterrence Failures,'" *ibid.*

[13] Jervis (fn. 3), 677-79.

[14] For further discussion, see George and Smoke, "Deterrence and Foreign Policy," *World Politics* 41 (January 1989), 170-82.

[15] Achen and Snidal's assertion that RDT has influenced policy then implies that it could not previously explain it: Achen and Snidal (fn. 6), 153. For a discussion of the alternative purposes of SEU models, see Schoemaker (fn. 5), 538-41.

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utility, but this says nothing about how they actually behave. In microeconomics, SEU theories can be both descriptive and prescriptive because of the argument that only those who behave in accordance with them can flourish. In international politics, the argument is even less convincing because the number of actors is much smaller, thus diminishing the power of the evolutionary logic.[16] Indeed, many RDT writings are clearly meant to influence statesmen, which implies that their behavior does not—or at least did not—follow the prescriptions.[17] The case-study literature, by contrast, is explicitly descriptive and explanatory, but usually implies that better results would have followed if statesmen had behaved differently. Indeed, as we shall see below, it often argues that deterrence is worse as prescription than as description: many misfortunes are attributed to statesmen following a particular version of it.

A related ambiguity is the level at which RDT applies: does it deal with national behavior, international outcomes, or both? If the former, what assumptions are the actors making about how the other will react? In some cases, effective behavior is premised on the adversary making an error, if not behaving irrationally. SEU and game theory may be compatible, but they are not identical. Turning to outcomes, it is clear that what is rational for the individual may be bad for the collective; the Prisoners' Dilemma further shows that if two people are rational, they can each be worse off than they would have been had both been irrational. If both sides act on RDT, will each be better off than if it had behaved differently?

Indeed, because it deals with strategic interaction, RDT may not be able to predict exactly how a state should behave or what international outcomes actions will produce. It is then hard to develop verifiable propositions from the theories. Economists have not been able to model the behavior of oligopolists nearly as deterministically as they have that of the wheat farmer facing a market he cannot influence. The game of Chicken

[16] But see Kenneth Waltz, *Theory of International Politics* (Reading, MA: Addison-Wesley, 1979). The application of the idea of evolutionary dynamics is not entirely straightforward. Traits with survival value do not automatically appear; they can survive even if they lack such value. A great deal depends on the harshness of the environment and the competition. For good discussions of evolution and SEU models, see Richard Nelson and Sydney Winter, *An Evolutionary Theory of Economic Change* (Cambridge: Harvard University Press, 1982), and Timothy McKeown, "The Limitations of 'Structural' Theories of Commercial Policy," *International Organization* 40 (Winter 1986), 52-55.

[17] Whether prescriptive, descriptive, or both, it is often unclear whether the claim is that the policy is an *appropriate* way to reach the person's goals or the *best* way to do so. The latter and stronger claim is obviously extremely difficult to verify since doing so requires comparing the results of the policy with those imputed to alternatives—in one version those policies the person thought of and in another version all those that could have been adopted. A strict interpretation of SEU would imply that the policy is the best one; most tests in social science more modestly seek to demonstrate that the policy is adequate for the goals.

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does not have a determinant solution; in many situations game theory prescribes a mixed or randomized strategy. Of course game theory yields great insights into how actors try to out-think and out-bluff each other, but the competitive and variable-sum nature of the situation means that scholars cannot produce deductions on the model: "In situation X the actor always should (or will) do Y." We can try to show that the actors' calculations are consistent with deterrence, but this requires looking into the "black box" of decision making.[18]

At this point, however, the greatest difficulty in assessing RDT is determining exactly what it is. As Achen and Snidal note, the general assumptions of SEU can yield an enormous number of substantive theories. There is, then, no *one* RDT: one can argue that there are at least as many theories as there are authors or that there are no theories because no one has developed one with sufficient rigor. Achen and Snidal's brief substantive discussion of deterrence illustrates the problem.[19] While it is true that some rational deterrence theorists have pointed to "the negative aspect of defensive systems such as civil defense, the ABM, and SDI," others reject this position, arguing that, unless the United States retains some ability to protect itself, its threats will lack credibility.[20] The disagreement is partly rooted in differences about the posture that is best for a country like the United States that needs to protect not only its homeland but also its allies. It is also caused by discrepant views of the goals and beliefs of the Soviet Union—questions that lie outside a general theory of deterrence. But what is important in this context is that both points of view can be modeled as RDTs.

This ambiguity makes the theory extremely hard to disconfirm. Are we to count the Reagan administration's advocacy of SDI as evidence against the validity of rational deterrence? Is the fact—if it is a fact—that the Soviet Union does not appreciate the virtues of mutual vulnerability evidence that the arms and doctrine of at least one of the superpowers cannot be explained by this theory? Is RDT confirmed by the fact that the Russians withdrew their missiles from Cuba, or disconfirmed because they deployed them in the first place—and were able to exact some concessions from the United States as the price for withdrawal?

[18] A good summary of relevant literature is Robert Wilson, "Deterrence in Oligopolistic Competition," in Stern et al., eds. (fn. 1).

[19] Achen and Snidal (fn. 6), 153.

[20] See, for example, Albert Wohlstetter, "Swords Without Shields," *National Interest*, No. 8 (Summer 1987), 31-57; Colin Gray, *Nuclear Strategy and National Style* (Lanham, MD: Hamilton Press, 1986). Part of the explanation for this dispute is a disagreement on whether decision makers focus on relative or absolute gains and losses. See Jervis (fn. 10, 1984), 59-63; Arthur Stein, *Dilemmas of Interdependence: Logics of International Conflict and Cooperation*, forthcoming.

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Empirical Criticisms and Second-Wave Deterrence Theory

We need to distinguish between the kinds of RDT that *could* be developed and those that *have* been most thoroughly discussed. Some of the criticisms raised by empirical studies[21] are aimed at the former; many, however, are aimed at the latter. The best-known and best-developed RDT—known as "classical" or "second-wave" deterrence theory[22]—makes a number of restrictive assumptions unrelated to rationality. These include: the state's adversary is highly motivated to expand; the adversary understands that the state is defensive; and the leaders on both sides are concerned mainly with the external situation. This partly explains why the focus is on threats and so little role is foreseen for inducements. Many findings contradict these auxiliary assumptions but could be accommodated by a broader RDT. Thus, Richard Ned Lebow and Janice Stein have demonstrated that statesmen may adopt deterrence policies that are not in the national interest because they are acting on the basis of their domestic or personal interests. For example, one reason why Nehru maintained the "forward policy" in 1962 against credible threats by China to take disputed territory was that he had staked his domestic political fortunes on standing firm.[23] While this kind of behavior does not fit the second-wave model, and is likely to increase conflict, there is nothing necessarily irrational about it (although, as we will note below, domestic pressures often lead to severely biased information processing).

Similarly, a rational theory can easily incorporate a second finding by the empirical critics: states may create a confrontation or go to war, not in the hope of making positive gains, but in order to avoid the losses that are foreseen unless they do so.[24] Expected utility theories of wars are sometimes summarized as asserting that states will fight only if the expected gains are greater than the expected costs, i.e., that the state will not fight if it thinks the results will be worse than the status quo. But this is an error: there is no reason why the state should believe that a future

[21] For convenience, I will treat these empirical studies as though the methods and results were entirely compatible with each other, which is not the case.

[22] For a discussion of the second wave, see Jervis (fn. 10, 1979), 291-301. It should also be noted that, while these writings are generally called a theory, they do not meet strict criteria for this designation.

[23] See Richard Ned Lebow, *Between Peace and War* (Baltimore, MD: The Johns Hopkins University Press, 1981); Robert Jervis, Richard Ned Lebow, and Janice Gross Stein, *Psychology and Deterrence* (Baltimore, MD: The Johns Hopkins University Press, 1985); Lebow and Stein (fn. 3). I am using the Nehru case, drawn from Richard Ned Lebow and Neville Maxwell, *India's China War* (New York: Pantheon, 1970), as a stylized illustration and am ignoring the complexities and ambiguities.

[24] For a similar argument, see Philip Tetlock, "Testing Deterrence Theory: Some Conceptual and Methodological Issues," *Journal of Social Issues* 43, No. 4 (1987), 85-92.

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without a war will be like the status quo. If it expects the future to be better, it can rationally seek to avoid a war that it thinks would yield significant benefits; if it expects the status quo to deteriorate badly, it can rationally undertake a war whose expected utility is much less than zero. The Japanese decision to attack Pearl Harbor and the German decision to go to war in 1914 may exemplify this type, although we should note again that this assumes the actors' probability and utility estimates, which in retrospect may appear to be bizarre.

But although rationality need not be in question here, the standard formulations of deterrence are. When the losses that the state fears stem from the adversary's threats and hostility (indicating the presence of a spiral process), further threats from the latter will rarely reduce the conflict. Thus, while an SEU model could be appropriate, the use of second-wave deterrence prescriptions would not in fact maximize utility.[25]

Nevertheless, one particular application of the idea that states could rationally fight because they feared a deterioration of the status quo if they did not stands as a central insight of second-wave theorists. They stressed the problem of "crisis instability," i.e., the danger that one side would launch a first strike, not because it was aggressive or believed that war was preferable to peace, but because it was sure that the other side was about to attack and believed that striking first was better than striking second.[26] Second-wave theorists could have broadened this insight into a general argument: a state defending the status quo must reassure the challenger that it is willing to respect the latter's vital interests, ensure that the adversary does not see it as aggressive, and minimize the security dilemma, i.e., minimize the extent to which the actions the state takes to increase its security decrease the adversary's security.[27] That second-wave theorists did not do so is attributable less to their general theories of international politics than to their belief that the Soviet Union understood that American military advantage would only be used to enhance deterrence, not to undermine Soviet security.

[25] For this reason, Lebow and Stein argue that the resulting theory would not be one of deterrence: "Rational Deterrence Theory: I Think, Therefore I Deter," *World Politics* 41 (January 1989), 212-14. Also see George and Smoke (fn. 14), 181-82.

[26] The classic statements are Albert Wohlstetter, "The Delicate Balance of Terror," *Foreign Affairs* 37 (January 1959), 211-34, and Thomas Schelling, "The Reciprocal Fear of Surprise Attack," in Schelling, *Strategy of Conflict* (Cambridge: Harvard University Press, 1960), chap. 9. Many previous wars developed because decision makers believed that the choice was between "war now and war later": Warner Schilling et al., *American Arms and a Changing Europe* (New York: Columbia University Press, 1973), 172-74. For an argument that the main danger of such dynamics today is largely psychological, see Robert Jervis, *Implications of the Nuclear Revolution* (Ithaca, NY: Cornell University Press, forthcoming), chap. 5.

[27] Indeed these points follow from a full SEU model: see John Mueller, *Retreat from Doomsday: The Obsolescence of Major War* (New York: Basic Books, forthcoming).

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Another finding is troublesome not only for the second-wave, but also for many other deterrence theories, even though rationality is again not at issue. The need for and possibilities of deterrence in peripheral areas depend in part on commitments being seen as interdependent. That is, the United States can credibly threaten to resist hostile moves anywhere because to refrain from doing so would undermine commitments throughout the globe. But it appears that others do not perceive commitments as so tightly linked and that the outcomes of confrontations are determined to a greater extent by each side's intrinsic interest in the issue at stake. The results can be modeled in terms of SEU, but many of the extensions of deterrence theory to low-level conflicts will have to be modified.

To return to the framework elaborated in the opening paragraph, second-wave theorists were almost exclusively worried that deterrence would fail because a status quo power lacked the strength, skill, or resolve to resist, or, even more likely, that an expansionist would incorrectly believe that the status quo power was weak in any of these ways, particularly the last-named. Theorists were not concerned that spirals of hostility and threat-perception could develop out of the unintended consequences of each side's behavior. Again, the reason is quite straightforward. Soviet expansionism was taken for granted.

It is, of course, perfectly legitimate for a theory of deterrence to take hostility as a given. Any theory has to start somewhere, and policy based on second-wave theory may be well designed to deal with certain kinds of expansionist states. But RDTs have no diagnostics; they say nothing about how statesmen are to determine others' intentions[28] and case studies reveal that if the other side seeks mainly security, deterrence can fail through the development of conflict spirals.[29] Second-wave theory may then provide a good description of American policy in the Cold War, but a misleading picture of the consequences of this policy. The United States may have used threats as the second wave recommends, but the results may have been to increase conflict and make the nation less secure.

The problems of both theorizing and policy making would be much easier if there were only one source of deterrence failure. But since the state cannot easily determine the adversary's intentions and how it will

[28] For a further discussion, see George and Smoke (fn. 11), 77-78; George and Smoke (fn. 14), 181-82; Jervis (fn. 10, 1979). The use of the term "diagnostic" in George and Smoke (fn. 14), 180 refers to the utility of deterrence theory in pointing out the situations in which deterrence is likely to be challenged, but the theory cannot tell statesmen whether they are facing such a situation.

[29] This also was the main point of earlier critics of the second wave: see, for example, Charles Osgood, *An Alternative to War or Surrender* (Urbana: University of Illinois Press, 1962).

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react to what the state does, it must guard against both appeasement and provocation. Decision makers, then, confront more pitfalls and have a wider range of choices than is true in the situation contemplated by the second wave. An RDT could maintain a stress on threats and force, but claim to apply only when a status quo state is faced with a rational, expansionist state. This would preserve much of the theory's power and parsimony at the cost of limiting its domain. Alternatively, RDT could be broadened to take account of a wider range of problems and more tools of statecraft. It could then cover more cases but would have to be much more complex. This complexity does not mean that rational and/or deductive theories are impossible, but the attempt to encompass both spiral and deterrence dynamics does mean that the resulting theory can neither be extremely parsimonious nor concerned predominantly with threats, force, and coercion.[30]

The findings presented so far call into question important parts of second-wave deterrence theory and show that there are multiple patterns, but they leave untouched the basic claims of SEU. After a brief discussion of a methodological problem, we will discuss the case-study arguments that are inconsistent with both standard deterrence and rational models.

Bias in Case Selection

Achen and Snidal have stressed that deterrence failures cannot be completely understood by studying only instances in which they occur, a point that, while frequently noted in the abstract, has often been ignored.[31] Social scientists generally begin—and often end—their research by locating examples in which the outcome of interest has occurred and then looking back in time for factors that seem to be linked to the outcome. This procedure tends to characterize studies of alliances, arms races, the procurement of weapons, civil unrest, and revolutions as well as of wars and de-

[30] Thus George has stressed the need to develop differentiated theories because cases are of different types: George and Smoke (fn. 11); Alexander L. George, Philip Farley, and Alexander Dallin, eds., *U.S.-Soviet Security Cooperation* (New York: Oxford University Press, 1988), chaps. 1 and 29; George and Smoke (fn. 14), 170-73. (A parallel argument from another field is that "no single theory is capable of explaining international trade in *all* commodities and at *all* times": Ali El-Agraa, *The Theory of International Trade* [London: Croom Helm, 1983], 85; also see Robert Gilpin, *The Political Economy of International Relations* [Princeton: Princeton University Press, 1987], 174-80.) For further discussion of why the theory would no longer be one of deterrence, see Lebow and Stein (fn. 25).

[31] Achen and Snidal (fn. 6), 160-63; also see Lebow and Stein (fn. 3), 7-8; George and Smoke (fn. 11), 516-17; Jervis, Lebow, and Stein (fn. 23), 13; Jervis (fn. 2), 368-70; Ariel Levite, *Intelligence and Strategic Surprises* (New York: Columbia University Press, 1987), 23-24. In *The War Trap* (New Haven: Yale University Press, 1981), Bruce Bueno de Mesquita argues that social science should seek only necessary conditions, in which case the problem does not arise. The limitations of this approach are obvious, however.

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terrence failures. It can neither yield information on the relative frequency of these events nor prove that the variables that are associated with the outcome are more than necessary conditions. That is, these variables may be present in many cases in which the outcome is different; indeed, in principle they could be universal.

Case selection in studies of deterrence failures presents some special problems. Even comparing cases in which crises led to war to those in which they did not will not cover all the relevant issues: while the latter are examples of success in what Patrick Morgan calls "immediate deterrence," in which the challenge is resisted without violence, they also are failures of "general deterrence," in which deterrence did not prevent the crisis from arising in the first place.[32] Even if we can determine the factors that discriminate between the failure and success of immediate deterrence, we cannot extend the argument to general deterrence. The very fact that a case of immediate deterrence arises means that the defender thought he had a defensible position and the challenger thought that he could get his way by force or coercion. The determinants of whether these conflicting expectations can be reconciled without violence are likely to be very different from the determinants of whether they develop at all.[33]

We may want to examine cases of successful general deterrence in order to estimate its success rate and determine what distinguishes successes from failures. Because we want to judge the influence of threats by asking what the outcome would have been if circumstances or policy had been different, we need either hypothetical or real comparisons. Lebow and Stein make a good start at using the former,[34] but the enterprise is obviously extremely difficult. Studying real cases of success is not much easier. It is not clear what the relevant sample should be. For example, if we examine only cases in which one state had a marked desire to challenge another, we exclude cases in which deterrence suppressed this desire. Neither is it obvious how often cases should be counted. If country A is deterred from attacking B for 10 years should this be one success, 10 successes (one for each year), or 3,650 (one for each day)? From this it follows that we probably could not calculate a success rate.

But, at least for some purposes, these computations are not necessary. We may care less about how *often* deterrence fails than *how* it fails. The

[32] Morgan, *Deterrence: A Conceptual Analysis* (Beverly Hills, CA: Sage, 1977), 31-43.

[33] Paul Huth and Bruce Russett, "Deterrence Failure and Crisis Escalation," *International Studies Quarterly* 32 (March 1988), at 30, acknowledge this, but some of their analysis then proceeds as though this were not the case, as the students in my graduate seminar pointed out to me.

[34] Lebow and Stein (fn. 3), 33-36.

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reason is twofold. First, a strong form of RDT is falsified if there are *any* cases in which the conditions posited as necessary and sufficient for deterrence are met and yet deterrence fails. Granted, one can argue that even the most rigorous social science theory is only probabilistic, but it is not clear that such an approach is compatible with the basic assumptions of SEU.[35] Second, a concern with how crises and wars have occurred in the past—and might occur in the future—leads us to ask why statesmen undertake seemingly self-defeating actions like challenging powerful, credible commitments or taking positions that cannot be sustained. For this purpose, frequencies are irrelevant although comparisons are still required.

The focus on immediate deterrence—both successes as well as failures—is troublesome, however, because it can lead us to miss perhaps the most important feature of the last quarter century: the paucity of superpower crises. Whether this has been caused by the potency of general deterrence, the "maturing" of either or both sides, or a decline in the conflict of interest between them is difficult to determine. But these questions cannot even be asked when we look only at how conflicts are resolved rather than at how often they occur. We have numberless books and articles on the Cuban missile crisis and almost none on the fact that this was the last such confrontation. Policy, as well as scholarship, has suffered as elites and the general public have not had to consider whether their standard beliefs about the Soviet Union and the Cold War should be called into question by the lack of Soviet challenges. It might also be noted that the common failure to notice nonevents—dogs that do not bark—can only be explained by psychological biases such as those examined below; rational decision makers would see their significance.

Generalizations from Case Studies

What have we learned from the case studies? To begin with, there appear to be real limits to our ability to generalize. Idiosyncracies are many and powerful; similar outcomes can be reached by many different paths and apparently similar initial conditions do not always yield the same results.[36] Partly for this reason and partly because of the limitations of the existing research, what we have now is more a list than a tight theory. But it is not without structure and coherence. Many of the generalizations produced by the case studies are linked; the inferred processes can gen-

[35] See George and Smoke (fn. 14), 179.
[36] See George and Smoke (fn. 14), 173-74 and George, Farley, and Dallin (fn. 30), 5, 11-12.

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erate additional propositions; and the arguments apply to a wide range of problems in addition to deterrence and deterrence failures. For example, the suboptimal decision making that produced the *Challenger* disaster and the shooting down of an Iranian airliner by the American warship *Vincennes* is anticipated by these findings.

Because most of these generalizations are familiar and space is limited, I will only sketch them, omitting supporting evidence.[37] They fall into three categories. The first deals with cognitive limitations on information processing, the second with what can be called "motivated biases," and the third stems from the fact that policy is often made and almost always implemented not by individuals but by organizations.

First, because the world is very complex and people's information-processing capabilities are sharply limited, we must all employ a number of "short-cuts to rationality," including many that go beyond those originally specified by Herbert Simon.[38] The beliefs people hold and the inferences they draw do not change quickly in response to information they receive; people—statesmen, scientists, and individuals in their everyday lives—tend to act in accordance with theories they already subscribe to rather than to fresh data.

A second cognitive bias is that beliefs tend to be strongly influenced by historical analogies to recent important cases that the person or his country has experienced firsthand. American policy in the Cold War—and second-wave deterrence theory—cannot be understood apart from the experience of Hitler.

Third, the role of accidents and confusion tends to be underestimated. Other states and alliances tend to be seen as much more centralized than they actually are.

Fourth, rather than integrating many values, without realizing it people often decide on the basis of a single important value dimension.

Fifth, the way states view others' capabilities and intentions is often strongly influenced by the way they view their own role and situation. What they think of themselves is a reference point by which they judge much else in the world.

A second set of generalizations deals with motivated biases, i.e., deviations from what is usually considered rationality that stem not from

[37] Supporting evidence and detailed arguments can be found in Glenn Snyder and Paul Diesing, *Conflict Among Nations* (Princeton: Princeton University Press, 1977); George and Smoke (fn. 11); Morgan (fn. 32); Lebow (fn. 23); Jervis (fn. 2); Jervis, Lebow, and Stein (fn. 23); Alexander L. George, David Hall, and William Simons, *The Limits of Coercive Diplomacy* (Boston: Little, Brown, 1971); Irving Janis and Leon Mann, *Decision Making* (New York: Free Press, 1977); and Ralph White, *Fearful Warriors* (New York: Free Press, 1984).

[38] Simon, *Models of Man* (New York: Wiley, 1957).

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purely cognitive limitations, but from psychological pressures and needs. Some of these overlap the cognitive biases and produce parallel or reinforcing effects.

First, and linked to the last cognitive bias, statesmen like to think well of their countries. They therefore believe that others will understand that they are following approved standards of international conduct and are not threatening others' legitimate interests.

Second, and related to the fourth cognitive bias, people tend to minimize perceived value trade-offs. A person who believes that a policy is better than the alternatives on one value dimension is likely to believe that it is better on other important, although logically unrelated, dimensions as well. In this way people develop views of the environment that avoid the psychological pain of believing that, for example, they must endanger large numbers of innocent lives in order to defend their nation's interests.

Third, and related to the previous point, when statesmen feel that the alternatives are bleak, they are likely to exaggerate the chances that the chosen policy will succeed. Furthermore, they will not gather the information or perform the analyses that could indicate that this policy is likely to fail.

Fourth, although the evidence on this point is slight, it seems that the pressures to misperceive the environment in comforting ways are greatest when the person is facing severe losses and/or when he is strongly influenced by a value that he would rather not recognize (e.g., his political stake).

The third set of generalizations brings out the role of domestic politics and organizational behavior, the latter of particular importance for how policies are implemented.

First, domestic politics and coalition building provide some of the explanation for why states become expansionist.[39] The state is not a unified actor and policies that treat it as one may be less than optimally effective.[40]

Second, policies are often implemented in ways that are very different from those ordered and expected by the decision makers. Statesmen often fail to realize this and thus are ignorant—or, worse yet, misinformed—about what has been done in the name of their state. Sometimes this can

[39] The best treatment is Jack Snyder, *Myths of Empire: Domestic Politics and Strategic Ideology* (Ithaca, NY: Cornell University Press, forthcoming).

[40] Recent studies of the origins and termination of the World War II conflict between America and Japan show in fascinating detail the opportunities that were missed when one country failed to perceive the nature of the coalitional struggles within its adversary: see Michael Barnhart, *Japan Prepares for Total War* (Ithaca, NY: Cornell University Press, 1987), and Leon Sigal, *Fighting to a Finish: The Politics of War Termination in the United States and Japan, 1945* (Ithaca, NY: Cornell University Press, 1988).

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strengthen deterrence; in other cases it will defeat it. Generally, these mechanisms increase both the extent to which each side sees the other as hostile and the chance that events will escape control.[41]

What are some of the implications of these findings for deterrence? First, in almost no interactions do two adversaries understand each other's goals, fears, means-ends beliefs, and perceptions. Empathy is difficult and usually lacking.[42] As a result, states sometimes fail to deploy threats that would benefit them and on other, probably more numerous, occasions employ threats that provoke rather than deter. States often underestimate the desperation that their adversaries feel and incorrectly believe that the others do not see them as a threat. Thus the interaction of the two sides' policies can increase conflict unnecessarily.

Second, the adversary will miss or misperceive many of the state's signals. The fine-tuning of policies and the subtle bargaining tactics that would be called for in a world of clear communication cannot work in our world; policies based on the assumption that they can will be ineffective if not counterproductive.[43]

Third, commitments by one actor that are objectively clear and credible (as measured by the perceptions of disinterested third parties) may not be perceived by another. Furthermore, in these cases the state making the commitment or threat is not likely to grasp the other's perceptions, believing instead that the other correctly understands the situation. The state is therefore likely to predict and interpret the other's behavior wrongly, thus defeating deterrence and increasing conflict.

Fourth, actors tend to overestimate the potency of threats and underestimate the utility of rewards and reassurances.

Fifth, threats and conciliation generally need to be combined, but their optimal mixture and timing is extremely difficult, in part because it depends on the adversary's goals and perceptions, which are hard for the state to discern.[44]

Sixth, the success of immediate deterrence can weaken general deter-

[41] See, for example, Graham Allison, *Essence of Decision* (Boston: Little, Brown, 1971); Richard Ned Lebow, *Nuclear Crisis Management* (Ithaca, NY: Cornell University Press, 1987); Scott Sagan, "Nuclear Alerts and Crisis Management," *International Security* 9 (Spring 1985), 99-139; Jervis (fn. 26), chap. 3.

[42] For conflicting arguments about the relative frequency of the errors of mirror-imaging and lack of empathy, see Walter Laqueur, *A World of Secrets* (New York: Basic Books, 1985), 190-94, 272-77; White (fn. 37), 160-67; Robert Jervis, "Beyond What the Facts Will Bear," *International Journal of Intelligence and Counterintelligence* 1 (Spring 1986), 146-48.

[43] For a nice example, see Wallace Thies, *When Governments Collide* (Berkeley: University of California Press, 1980).

[44] See George, Hall, and Simons (fn. 37); Snyder and Diesing (fn. 37); Jervis (fn. 10, 1979); Janice Stein, "Deterrence and Reassurance," in Philip Tetlock et al., eds., *Behavior, Society, and Nuclear War*, Vol. 2 (New York: Oxford University Press, forthcoming).

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rence in the future by increasing the loser's grievances, convincing it that the state is a grave menace, increasing its incentives to stand firm in the next confrontation, or even leading it to fight in order to change a situation which has become intolerable or is expected to deteriorate.[45] Successful coercion may encourage the other side to build up its arms. It is possible, for example, that U.S. nuclear threats in the Taiwan straits crises of 1954-1955 and 1958 may have contributed to China's decision to develop nuclear weapons and that its more muted threats to India in 1971 may have similarly affected that country's decision to proceed with a nuclear test.

What Cannot Be Explained by a Rational Theory

The central question here is whether the empirical findings can be explained by an RDT, albeit one that has not yet been developed. The answer may turn in part on whether SEU takes the actors' beliefs as givens, as it does goals and utilities, or asks if they are reasonable.[46] For example, can we construct an SEU account of a person who eats three pounds of broccoli a day in the belief that this wards off dragons? What if the U.S. policy of intervening in countries like the Dominican Republic and Vietnam makes sense on the premise that the revolutions there were Soviet-inspired and that their success would undermine U.S. influence around the world, but we think that these expectations are not only false, but clearly contradicted by the evidence? Can we apply SEU if the policy fails year after year because the supporting beliefs are consistently incorrect? It will be difficult to verify the theory if we are required to judge whether the beliefs are reasonable, but if we accept all beliefs, however outrageous, we will have stretched the theory at the expense of common sense and utility. Furthermore, in order to argue that the behavior made sense in terms of SEU we may have to ignore people's accounts of their own reasons and goals. For example, if U.S. policy had the effect of consistently increasing radicalism and anti-U.S. feeling in the third world, would we judge it as rational if statesmen told us that their intention was exactly the opposite?

Putting aside the rationality of the substance of the beliefs, it can be claimed that the cognitive biases are consistent with rational behavior properly understood.[47] Information and information processing are costs;

[45] For a parallel argument, see George and Smoke (fn. 14), 182.

[46] For discussions of this issue, see Jon Elster, "Introduction," in Elster, ed., *Rational Choice* (New York: New York University Press, 1986), 4-5, 13-16; Barry O'Neill, "Game Theory and the Study of the Deterrence of War," in Stern (fn. 1); Lebow and Stein (fn. 25), 216-18.

[47] The familiar analogy to athletes, put forward by Milton Friedman and Leonard Savage, "The Utility Analysis of Choices Involving Risk," *Journal of Political Economy* 56 (August

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people maximize utility when this is taken into consideration.[48] Thus SEU is not disturbed by the fact that people maintain incorrect beliefs that support failing policies well past the point at which they have sufficient information that would reveal this failure to anyone not committed to those beliefs at the outset. A full consideration of each bit of new information and the constant reconsideration of images and beliefs would be impossibly costly. Although second-wave theory is disturbed by, for example, the case in which an objectively credible threat is ignored because the perceiver did not expect it, an SEU account considering information-processing costs would not be.

This argument has some value, but the stretching of the normal meaning of subjective utility maximization is not its only problem. First, it assumes that people somehow stop gathering and processing information just at the point at which the costs outweigh the expected gains. It is not clear how this point could be estimated—indeed trying to do so would itself be very costly. Second, this argument implies that cognitive biases should operate much less when the decision is very important; but this has not been demonstrated.[49] Indeed, it has been studies of crucial choices that have revealed the role of these biases. Neither Japan in 1941 nor Nehru's India in 1962 gathered a great deal of information on how their adversaries would be likely to react. Similarly, although much of Britain's policy during the 1930s rested on beliefs about the efficacy of strategic bombardment, the supporting analyses were few and weak.[50] Third, it is far from clear that this approach can capture all the shortcuts to rationality that operate, let alone anticipate ones not yet discovered. For example, this explanation probably cannot account for the methods by which people see a case as fitting into a general category and the different

1948) and Achen and Snidal (fn. 6), 164, does not really help. Statesmen act in a realm of much greater complexity, deal with many fewer instances, and only rarely receive unambiguous feedback. For a discussion of the more interesting issue of whether information-processing biases and fallacies really can be considered deviations from rationality, see L. Jonathan Cohen, "Can Human Irrationality be Experimentally Demonstrated?" *Behavioral and Brain Sciences* 4 (September 1981), 317-31, and the comments, *ibid.*, 331-70. For an SEU model that takes information-processing costs into account, see Paul Anderson and Timothy McKeown, "Changing Aspirations, Limited Attention, and War," *World Politics* 40 (October 1987), 1-29.

[48] William Riker and Peter Ordeshook, *An Introduction to Positive Political Theory* (Englewood Cliffs, NJ: Prentice-Hall, 1973); Anthony Downs, *An Economic Theory of Democracy* (New York: Harper & Row, 1957). Also see Jervis (fn. 2), chap. 4.

[49] For some relevant findings, see David Sears and Jonathan Freedman, "Selective Exposure to Information: A Critical Review," *Public Opinion Quarterly* 31 (Summer 1967), 194-213; William McGuire, "Selective Exposure: A Summing Up," in Robert Abelson et al., eds., *Theories of Cognitive Consistency* (Chicago: Rand McNally, 1968), 797-800; Philip Tetlock, "Accountability: The Neglected Social Context of Judgment and Choice," in Barry M. Staw and L. L. Cummings, eds., *Research in Organizational Behavior*, Vol. 7 (Greenwich, CT: JAI Press, 1985).

[50] Robert Jervis, "Deterrence and Perception," *International Security* 7 (Winter 1982/83), 14-17.

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weights that people give to different kinds of information. Nor can it account for the fact that people deny the validity of many of the laws of probability.[51]

Of course one reply is that none of this is relevant: we want a model that will match most of the states' behavior, not reconstruct the decision-making process. But it is hard to see how SEU will in fact succeed in doing so in light of the influences we have listed. Not only do we observe statesmen failing to pay attention to factors necessary for rational calculation, but in many instances the reasoning processes they report—which track nicely with laboratory results in which people actually make choices—involve errors of inference and calculation. Only if these were somehow to cancel out could the results fit with SEU. Indeed, one must wonder whether intricate and counterintuitive SEU arguments can explain statesmen's behavior. Decision makers will only adopt courses of action that they expect to be effective; if the likely advantages can only be discovered or demonstrated with the aid of complex mathematics it would seem unlikely that statesmen would see the advantages of the behavior. Furthermore, in many cases, the other side's perceptions are crucial. Thus if we could show that, contrary to what is almost universally believed, a certain situation or tactic should give the state a bargaining advantage, this will actually be true only if the other side realizes it and so in fact is more willing to make concessions when faced with that situation or tactic.

The argument that people's behavior can be explained by assuming that they act as though they were rational may cope somewhat better with motivated biases. When these operate, the external environment is distorted to serve psychological or political functions.[52] But the fact that the policy owes little to a disinterested appreciation of the external environment does not mean that it cannot be explained by SEU. To return to an example mentioned earlier, the fact that Nehru avoided the psychic pain that would have resulted from the recognition that he was putting his political fortunes ahead of the good of the country is interesting for a psychologist, but it is irrelevant to the argument that we can explain India's policy as though India were acting to maximize Nehru's personal utility. Furthermore, if a person has no choice but to undertake a course of action, the motivated distortions may not influence her behavior at all.[53]

[51] See, for example, the work summarized in Daniel Kahneman, Paul Slovic, and Amos Tversky, eds., *Judgment under Uncertainty: Heuristics and Biases* (Cambridge: Cambridge University Press, 1982), and Tversky and Kahneman, "Rational Choice and the Framing of Decisions," in *Journal of Business* (fn. 5), S251-78.

[52] The classic account is M. Brewster Smith, Jerome Bruner, and Robert White, *Opinions and Personality* (New York: Wiley, 1956).

[53] For further discussion, see Jervis (fn. 2), 374-78; and Arthur Stein, "When Misperception Matters," *World Politics* 34 (July 1982), 505-26.

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The fact that she badly underestimated the risks or costs of her policy does not mean that she would have behaved differently had her analysis been accurate. For example, in 1941, motivated bias may have led Japanese decision makers to overestimate the chances that the United States was willing to wage—and lose—a limited war, but if their utilities left them no choice other than to fight, this error may have only rationalized, not influenced, their behavior.

Even when motivated biases produce more complex and seemingly irrational behavior, an SEU theory might still hold. In an older sense than Schelling's, there may be rationality in irrationality.[54] We usually say that a person who consistently acts in self-defeating ways is probably driven by subconscious ego dynamics. But this is not so different from saying that the costs of sacrificing certain goals are more than compensated for by the avoidance of the pain that would come from directly facing the unconscious impulses coupled with the gains on other value dimensions (e.g., the misery caused by the person's actions can gratify deeply felt needs to be debased).

This could even be the case when the person would have been better off had he followed an alternative policy that he could have discovered with the available data. This is a stronger challenge to SEU because the policy chosen does not maximize even the hidden utilities. To continue with the Nehru example, assume that if he had understood the situation, he could have developed a policy that he would have seen as likely to produce a better outcome. In this case the motivated bias would have led to suboptimal behavior: had his vision not been clouded by fears and needs, he could have managed the trade-off between national and personal interest better. And yet an SEU account is still possible once we consider the pain Nehru would have suffered had he confronted the value conflict and his own ambitions. We might similarly deal with behavior that follows from personal, class, or national ideologies that provide a justification for a useful policy by developing a distorted view of the external environment. The result is to purchase immediate individual and social harmony at the cost of a later disaster not only for the country as a whole, but also for the controlling individuals and groups. The beliefs and policies of Wilhelmine Germany provide an example: the country and ruling elite were held together at the expense of a realistic appraisal of the threats and opportunities that others offered. In cases like this, the bargain is not struck consciously and in retrospect may appear to be a bad one, but it can be claimed to be compatible with SEU.

[54] Schelling (fn. 26), 36-43.

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To put this another way, the immediate gratification provided by a motivated bias could outweigh the anticipated long-run cost that would be incurred when the policy eventually fails. People may have high discount rates (i.e., what will happen in the future counts far less than immediate consequences) and part of their utility can be peace of mind and a desired self-image. For example, the fact that deterrence fails because the statesman avoids seeing that his policy is provoking rather than restraining the adversary can be incorporated into SEU once we consider the utility to the leader and the country as a whole of the belief that it is not a menace and could not be seen as such.

I suspect that many readers will find this argument as tortuous and unsatisfactory as I do. It is the equivalent of saying that the person who puts off seeing a doctor for fear that her lump will be diagnosed as cancer is rational once we consider the immediate costs of getting bad news. Any theory that has to take account of utilities like these will be far removed from any RDT that has been advanced. Furthermore, this kind of theory could yield deductions only if people are consistent in their discount rates and in the subjective costs of extended searches and knowledge of their own utilities. But even if these requirements are met, the behavior still might not be rational—by some definitions—if the short-run gain is purchased at the price of greatly limiting or harming the actor's future prospects.[55]

At least as difficult for most notions of SEU are the cases in which policy is rationalized in a way that changes later perceptions and utilities. For example, assume that domestic politics leads a statesman to adopt a "hard line" policy and that he then reduces the trade-off between domestic and foreign policy values by coming to believe that it is the latter that requires this policy. But if the domestic environment changes, the policy may continue because it is supported by the newly created perceptions of the foreign environment.[56] What utilities are then being maximized?

Is Behavior Consistent?

The minimum requirement of SEU is that behavior and thus the inferred preferences be consistent. But we know that a nation—like any collective—may act inconsistently although each person in it is fully consistent, thus making an SEU explanation of the former but not the latter

[55] Elster (fn. 46), 10-12.

[56] For related arguments, see Jon Elster, *Sour Grapes: Studies in the Subversion of Rationality* (Cambridge: Cambridge University Press, 1983), chaps. 3 and 4, and Deborah Larson, *Origins of Containment* (Princeton: Princeton University Press, 1985).

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impossible. Few foreign policy case studies have examined this question rigorously, and more attention has been paid to it in the area of political economy than in security. In the latter area it is generally assumed that the compelling nature of the external environment and the importance of the national interest submerge divergent domestic interests and coalition dynamics that could produce inconsistent policies. But certainly this is not always the case.

Internal bargaining can undermine the assumption that the state acts as a rational unitary actor in several ways. First, some coalitions can be built and maintained only by including policy elements which pull in opposite directions. For example, splits in the British cabinet before World War I meant that a moral commitment was incurred to stand by France in the event of an attack by Germany, but that Germany could not be so informed. The result was a parody of deterrence prescriptions—France felt emboldened by the expectation of British support; Germany was emboldened by overestimating the chance that Britain would remain neutral. After the war, similar splits led Britain to give the White Russians just enough support to ensure that the Bolsheviks would be enraged, but not enough to overthrow the new regime. Governments that are deeply divided often "do things by twos." That is, they are likely to maintain domestic unity by making paired decisions, although the resulting actions will contradict or undercut each other.[57]

Second, disagreements can be so deep that no decision is possible, especially when close to consensus is required. Thus, the Japanese government remained deadlocked over the question of whether to accede to Germany's desire to strengthen the Anti-Comintern Pact despite the considerable costs of the year-long procrastination and the fact that Japan was unable to respond to the changes in the external environment during that period.[58]

Third, inconsistencies over time appear as first one faction and then another comes to power. Even if broad national goals remain because they are set by the external environment, disagreements on how to reach them are deep and frequent. Thus while much of American foreign policy during the cold war can be characterized as containment, the supporting behaviors varied as different administrations came to power with different ideas and different priorities for the use of resources.[59] It is, then,

[57] Warner Schilling, "The Politics of National Defense: Fiscal 1950," in Warner Schilling, Paul Hammond, and Glenn Snyder, *Strategy, Politics, and Defense Budgets* (New York: Columbia University Press, 1962), 1-266; Thies (fn. 43).

[58] Ohata Tokushiro, "The Anti-Comintern Pact, 1935-1939," in James Morley, ed., *Deterrent Diplomacy: Japan, Germany, and the USSR, 1935-1940* (New York: Columbia University Press, 1976), 47-111.

[59] See John Gaddis, *Strategies of Containment* (New York: Oxford University Press, 1982).

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with justification that allies and adversaries alike complain that they are constantly forced to adjust to unpredictable changes in American policies generated by shifting domestic politics. Of course, one can simply treat each administration as a different actor with different preferences. One can similarly accommodate the changes during an administration produced by shifting alignments. But the resulting theories will not be very parsimonious and powerful.

Fourth, while the nature of the decision-making process makes it hard to demonstrate the intransitivity of preferences, the fact that policy often arises from the interaction of several factions makes cyclical majorities quite likely. It is possible that the instability of recent U.S. foreign policy can be explained in part by the division of elite opinion makers into cold war internationalists, post-cold war internationalists, and semi-isolationists.[60] Similarly, opinion over the wars in Korea and Vietnam was not arrayed on a simple hawk-dove continuum, which would at least have yielded a coherent policy, but rather divided into three groups with different orders of preference for the alternatives of escalating, continuing a limited war, and withdrawing.[61]

Finally, in the formation and, even more, the implementation of policy, divergent bureaucratic interests and perspectives often remain unintegrated and the resulting "national" policy is inconsistent. In July 1914 the civilian and military leaders in Germany sent conflicting instructions to Vienna, inducing the leaders of Austria-Hungary to ask "Who rules in Berlin?"[62] In other cases, different elements of the government work separately on what they think are unrelated problems, but the outcomes have contradictory impacts on foreign governments. For example, when some parts of the U.S. government were trying to discourage third world states from controlling the full nuclear fuel cycle (and thus gaining ready access to fissile material that could be used for bombs), the agency that was dealing with the capacity and safety of U.S. plutonium separation plants cut off foreign contracts for reprocessing nuclear fuel, thereby inadvertently showing others that the United States could not be counted on to meet their needs. Neither group was engaged in a struggle with the other for control of policy; instead each just carried out its responsibilities

[60] Ole Holsti and James Rosenau, *American Leadership in World Affairs: Vietnam and the Breakdown of Consensus* (Boston: Allen & Unwin, 1984).

[61] Andre Modigliani, "Hawks and Doves, Isolationism and Political Distrust," *American Political Science Review* 66 (September 1972), 960-78. For rational choice discussions of how institutions can provide solutions to cyclical majorities, see Kenneth Shepsle and Barry Weingast, "Political Solutions to Market Problems," *American Political Science Review* 78 (March 1984), 417-34; and Kenneth Shepsle and Barry Weingast, "Structure Induced Equilibrium and Legislative Choice," *Public Choice* 37, No. 3 (1982), 503-19.

[62] Japanese and U.S. policies at the end of World War II provide several examples; see Sigal (fn. 40).

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in its own sphere without much awareness of the total policy implications—a situation which ended up undermining rather than maximizing national utilities.

Furthermore, even if nations acted as though they were individuals, problems might arise: in their everyday lives people reveal inconsistencies that violate SEU. This is really not surprising. Individuals, despite their skin, may be no more unified than large and segmented organizations. Of course, social life would be impossible if behavior were random, but we should not exaggerate the degree of consistency that prevails. Thus, SEU models of consumer behavior have not been striking successes; political preferences and utilities change frequently in ways that do not easily fit standard rationality;[63] in seeking multiple goals, people often act in contradictory ways. Recent psychological research has also demonstrated that people choose differently depending on how the question is framed. Consider these examples. First, you lose a $20 ticket while walking to the theater; alternatively, as you go to purchase your ticket, you find that you have lost a $20 bill from your wallet. You are probably more likely to buy a ticket in the second case than repurchase it in the first, although the choices are formally identical. Second, you are about to buy a $25 calculator when you hear that the same item is available for $20 in another store a few blocks away; the same situation holds true except that the item is a television set and the respective prices are $500 and $495. You are more likely to save the $5 in the first situation than the second. Finally, "Mr. R bought a case of good wine in the late 50's for about $5 a bottle. A few years later his wine merchant offered to buy the wine back for $100 a bottle. He refused, even though he has never paid more than $35 for a bottle of wine." These effects have been demonstrated in the laboratory and the same processes probably operate in foreign policy making.[64] For example, if statesmen judge policies by comparing the expected results with the status quo, as they seem to, they may run high risks rather than accept the certainty of a loss that is small compared to their overall value

[63] See Robert Jervis, "Realism, Game Theory, and Cooperation," *World Politics* 40 (April 1988), 324-28, 340-44.

[64] The wine example comes from Richard Thaler, "Toward a Positive Theory of Consumer Choice," *Journal of Economic Behavior and Organization* 1 (March 1980), 43; the other examples are from Amos Tversky and Daniel Kahneman, "The Framing of Decisions and the Psychology of Choice," *Science* 211 (January 1981), 453-58. Also see David Grether and Charles Plott, "Economic Theory of Choice and the Preference Reversal Phenomenon," *American Economic Review* 69 (September 1979), 623-38; Tversky and Kahneman (fn. 51); Jack Knetsch, Richard Thaler, and Daniel Kahneman, "Experimental Tests of the Endowment Effect and the Coase Theorem" (unpub.); Kenneth MacCrimmon and Donald Wehrung, *Taking Risks* (New York: Free Press, 1986). For applications to foreign policy, see Jervis (fn. 26), chap. 5, and Nancy Kanwisher, "Cognitive Heuristics and American Security Policy," *Journal of Conflict Resolution* (forthcoming).

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position. The policies will then be inconsistent with other behavior and defy many normal deterrence prescriptions. Similarly, when people reduce cognitive dissonance by coming to see the chosen alternative as even better than they saw it when they reached their decision, their later behavior will be influenced in a way that makes it inconsistent with their earlier preferences. Biased information processing, denigrating rejected courses of action, and bolstering the selected one not only make the person more comfortable with his decision, they also change the way he sees the world and the value he places on various outcomes. If SEU theories are to be developed further, they will have to do a better job of coming to grips with the theoretical and empirical questions of the consistency of national behavior, taking into account both the coalition nature of politics and the operation of individual psychology.

SUMMARY

Even if we put some of these difficulties aside, in any SEU that will fit the empirical findings, the subjective elements will loom large. This is true not only for values and utilities, but also for the crucial means-ends beliefs, perceptions of the other side, and estimates of the probable consequences of alternative policies. These factors are all exogenous, and studying them in individual cases, trying to see how and why they form, and locating whatever patterns exist are crucial tasks. Propositions on these subjects constitute much of the work being done by SEU explanations.[65] Of course, deductive theories can play a role here, but assertions that people act as though they were maximizing utility will not help answer many of the hard questions.

[65] See Herbert Simon, "Human Nature in Politics: The Dialogue of Psychology and Political Science," *American Political Science Review* 79 (June 1985), 292-303; Jervis (fn. 63), 324-29.

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RATIONAL DETERRENCE THEORY: I Think, Therefore I Deter

By RICHARD NED LEBOW and JANICE GROSS STEIN*

> . . . abstract models are like scaffolding used to build a structure; the structure must stand by itself. If the abstract models contain empirical falsities, we must jettison the models, not gloss over their inadequacies.
>
> —*Paul Samuelson*[1]

PROPONENTS of "rational deterrence theory" contend that it is conceptually sound, a good predictor of strategic behavior, and a successful strategy of conflict management. All three assertions are unwarranted. Existing theories of deterrence are incomplete and flawed. They are poor predictors of critical cases of strategic behavior and equally poor guides to policy. Theories of deterrence do not consider the most important determinants of strategic choice. These determinants are outside of and at times contradictory to their fundamental assumptions. To develop theories with predictive capability and policy relevance, scholars must go beyond deterrence and rational choice to other theories of international behavior.

Theories of Rational Choice

Theories of rational choice assume that people maximize expected utility. But rational-choice theorists recognize that utility can be calculated differently by different but equally rational actors.[2] Some people may rationally minimize expected losses while others may maximize expected gains. People can also be risk-prone or risk-averse without deviating

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[1] Samuelson, "Problems of Methodology—Discussion," *American Economic Review* 53 (May 1963), 236.

[2] This problem is discussed by Frank C. Zagare in "Recent Advances in Game Theory and Political Science," in Samuel Long, ed., *Annual Review of Political Science* (Norwood, NJ: Ablex Publishing Corporation, 1986), 60-90, and in *The Dynamics of Deterrence* (Chicago: University of Chicago Press, 1987), 1-7. It is also a central theme of Arthur Stein, *Dilemmas of Interdependence: The Logic of International Cooperation and Conflict*, forthcoming.

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from the norm of rationality.[3] Subjective expected utility will vary enormously depending on actors' risk propensity and relative emphasis on loss or gain.[4] Risk-prone, gain-maximizing leaders—Hitler comes readily to mind—make very different choices than their risk-averse, loss-minimizing counterparts.

Models of rational choice must therefore build in assumptions about the risk propensities of players and the relative weighting of gains and losses. To do so they must turn to other theories of behavior.[5] As Herbert Simon has pointed out, a rational political theory is always more than rational utility theory. The principle of rationality says very little that is interesting or important.[6] It only takes on meaning through auxiliary propositions that can provide the basis for a deductive theory of political behavior.[7]

Theories of Deterrence

Some theories of deterrence purport to supply the additional assumptions rational-choice theories need to predict rational strategic behavior. Theories of deterrence generally assume that would-be initiators are risk-prone gain maximizers. Christopher Achen and Duncan Snidal properly describe the principal explanatory assumption of deterrence theories as the expectation that variation in outcomes can be explained by differences in actors' opportunities.[8] The fundamental theoretical concern of deter-

[3] Formal concepts of risk and uncertainty apply to the estimation of the likelihoods of given alternatives. In a "risky" choice, the probability distribution of a consequence is known, even though its occurrence is not. In an uncertain environment, by contrast, even this distribution of probabilities is unknown and no a priori estimates can be made. The formal distinction between risk and uncertainty does not capture the attributes of the environment of strategic decision making. Once leaders estimate the probability of a consequence of an option, even without adequate baseline information on which to make their judgments, they make their choice under conditions of risk.

[4] In models of subjective expected utility, preferences under conditions of risk are determined by at least two separate factors: the strength of the preference for the consequences under certainty, and the attitude toward risk. See Paul J. H. Schoemaker, "The Expected Utility Model: Its Variants, Purposes, Evidence, and Limitations," *Journal of Economic Literature* 20 (June 1982), 529-63, at 533.

[5] Joseph S. Nye, Jr., develops this argument in "Neorealism and Neoliberalism," *World Politics* 40 (January 1988), 235-51.

[6] Simon, "Human Nature in Politics: The Dialogue of Psychology with Political Science," *American Political Science Review* 79 (June 1985), 293-304.

[7] For a cogent exposition of this argument, see Dwain Mefford, "An Alternative to Rational Choice: The Function of Stories in the Formulation of Foreign Policy with Examples from U.S. Intervention," paper delivered at the MIT-Harvard seminar, "Rethinking Security Relationships and International Institutions and Cooperation," Cambridge, MA, May 5, 1988.

[8] Achen and Snidal, "Rational Deterrence Theory and Comparative Case Studies," *World Politics* 41 (January 1989), 150.

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rence has therefore been a specification of the ways in which defenders can use threats to increase leaders' calculations of the likely costs of a challenge.

This assumption of deterrence is arbitrary. It ignores the three other logical possibilities: initiators could also be risk-prone loss minimizers, risk-averse gain maximizers, and risk-averse loss minimizers. They are not all like Adolf Hitler. Nikita Khrushchev was risk-prone but appears to have emphasized loss, as were Japan's leaders in 1941, and Anwar el-Sadat in 1973.[9] Joseph Stalin is probably best described as a risk-averse gain maximizer.[10]

Empirical analyses of deterrence failures indicate that risk-prone gain-maximizing initiators—the assumption of almost all theories of deterrence—are relatively uncommon. The most serious challenges to deterrence most often come from initiators who are risk-prone but emphasize loss. Case studies indicate that outside threats have no effect on the calculations of this kind of initiator or may make a challenge even *more* attractive.[11]

[9] See Richard Ned Lebow, "Provocative Deterrence: A New Look at the Cuban Missile Crisis," *Arms Control Today* 18 (July-August 1988), 15-16, for recent Soviet revelations about Khrushchev's motives for the Cuban missile deployment. Earlier Western studies that emphasized Khrushchev's concern for avoiding loss include, Arnold Horelick and Myron Rush, *Strategic Power and Soviet Foreign Policy* (Chicago: University of Chicago Press, 1966), 141; Roger Hilsman, *To Move A Nation: The Politics of Foreign Policy in the Administration of John F. Kennedy* (Garden City, NY: Doubleday, 1967), 200-202; and Jerome H. Kahan and Anne K. Long, "The Cuban Missile Crisis: A Study of Its Strategic Context," *Political Science Quarterly* 87 (December 1972), 564-90. On Japan, see Robert Butow, *Tojo and the Coming of the War* (Stanford: Stanford University Press, 1961); N. Ike, *Japan's Decision for War, Records of 1941: Policy Conferences* (Stanford: Stanford University Press, 1967); C. Hosoya, "Miscalculations in Deterrence Policy: Japanese-U.S. Relations, 1938-1941," *Journal of Peace Research* 5, No. 2 (1968), 97-115; Bruce Russett, "Deterrence Theory and Decision Theory," *Journal of Peace Research* 4, No. 2 (1967), 89-105; D. Borg and S. Okamoto, eds., *Pearl Harbor as History: Japanese-American Relations, 1931-1941* (New York: Columbia University Press, 1973); and S. Ienaga, *The Pacific War, 1931-1945* (New York: Pantheon, 1978). On Sadat, see Janice Gross Stein, "Calculation, Miscalculation, and Conventional Deterrence I: The View from Cairo," in Robert Jervis, Richard Ned Lebow, and Janice Gross Stein, *Psychology and Deterrence* (Baltimore: The Johns Hopkins University Press, 1985), 34-59.

[10] See Vojtech Mastny, *Russia's Road to the Cold War: Diplomacy, Warfare, and the Politics of Communism, 1941-45* (New York: Columbia University Press, 1979), and William Taubman, *Stalin's America Policy: From Entente to Detente to Cold War* (New York: Norton, 1982), for this interpretation.

[11] For an analysis of the conditions under which the strategy of deterrence is likely to provoke rather than prevent a use of force, see Robert Jervis, *Perception and Misperception in International Politics* (Princeton: Princeton University Press, 1976), 58-113, and "Rational Deterrence: Theory and Evidence," *World Politics* 41 (January 1989), 183-207; Richard Ned Lebow, *Between Peace and War: The Nature of International Crisis* (Baltimore: The Johns Hopkins University Press, 1981), and "The Cuban Missile Crisis: Reading the Lessons Correctly," *Political Science Quarterly* 98 (Fall 1983), 431-58; Richard Ned Lebow and Janice Gross Stein, "Beyond Deterrence," *Journal of Social Issues* 43, No. 4 (1987), 3-71; and Janice Gross Stein, "Deterrence and Miscalculated Escalation: The Outbreak of War in 1967," paper presented to the American Political Science Association, September 1988.

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Deductive theories of deterrence that assume that challengers are risk-prone gain maximizers will not only be poor predictors of outcomes but will also create serious problems for those who use the theory as a guide to strategy. Recognizing this problem, some analysts have begun to experiment with varying assumptions of risk propensity in their models of subjective expected utility.[12] If these efforts are to be grounded in theory, analysts must look beyond deterrence to other theories of international behavior for guidance. Ultimately, however, the risk propensity of challengers and their relative weighting of loss and gain are empirical questions that can be answered only by careful historical research.

Deductive theories of deterrence also err in their specification of the critical factors that influence a challenger's calculus. Achen and Snidal, once again representative of deterrence theorists, argue that the calculus of deterrence is straightforward and undemanding: "If a country knows that it is likely to lose a long, nasty war in the process, it will probably not seek to press its claims against a rival. The trick is to learn the likelihood that the rival country will fight—and if it fights, how likely it is to win." More formally, they suggest that an initiator's choices depend upon its subjective estimates of the costs of war, the probability of winning, and the estimated probability that the defender will retaliate.[13]

Empirical analyses of deterrence failures have identified cases in which leaders calculated in accordance with the expectations of deterrence theories but acted contrary to their predictions. These leaders estimated the expected costs of war as very high, the probability of winning as low, and the probability that the defender would retaliate as virtually certain. Yet, they chose to challenge deterrence. Japan's leaders made these kinds of calculations when they chose to attack the United States in 1941, as did Sadat when he decided to challenge Israel in 1973. Contrary to the assertion of Achen and Snidal, cases of this kind do constitute a partial test of the predictive validity of "rational deterrence theory." They demonstrate that the interrelationships among the variables specified by the theory did not produce the anticipated outcome.

A close examination of these and other cases of deterrence failure indicates that the calculus of initiators depends on factors other than those identified by deterrence.[14] Perhaps the most important of these is the expected domestic political consequences of a use of force. For Sadat in

[12] Bruce Bueno de Mesquita and David Lalman, "Reason and War," *American Political Science Review* 80 (December 1986), 1113-30; Stein (fn. 2).

[13] Achen and Snidal (fn. 8), 151 and n. 24.

[14] These cases are analyzed in Jervis, Lebow, and Stein (fn. 9), 34-59, 203-32, and in Lebow and Stein (fn. 11).

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1973, the critical consideration—and a variable omitted from deterrence theories—was his estimate of the adverse economic and political consequences of inaction.

Rational deterrence theories do more than omit important variables. Theories of deterrence focus on the manipulation of an initiator's estimates of the costs of war, the probability of winning, and the probability of retaliation through credible threats reinforced by usable military options. Once analysts admit the critical relevance of other variables, it becomes necessary to determine the relative weighting of these variables or the circumstances in which one or another dominates calculations of utility. Achen and Snidal admit the importance of domestic politics but neither they nor theories of deterrence provide a basis for the specification and weighting of this and other omitted variables. The relative importance of the domestic consequences of action or inaction cannot be derived from any of the assumptions, postulates, or axioms of "rational deterrence theory," no matter how they are reformulated. We must once again turn to other theories of international behavior.

Can "Rational Deterrence Theory" Be Saved?

How do proponents of "rational deterrence theory" accommodate evidence of its failure to predict outcomes in important cases? They acknowledge its weaknesses, relax critical assumptions, and fall back on a broad definition of net subjective expected utility as the predictor of deterrence outcomes. Achen and Snidal, like many others, assert that the initiator's subjective expected utility of attacking must be less than that of continuing with the status quo.[15] This is an unworkable theoretical formulation.

Whether or not an actor is rational is beside the point. Deterrence theory does not predict that initiators will be rational. It specifies the conditions under which rational initiators will choose not to attack. The two are not the same. Deterrence theory derives its rigor and predictive power from its specification of the interrelationship among the critical variables that allegedly shape an initiator's calculus. If its claim to theory is to be taken seriously, the variables it identifies and their interaction must have the anticipated outcome. When a challenger makes a decision to use or not to use force with reference to criteria outside of deterrence theory—such as domestic or alliance politics—then the validity of deterrence theory is doubtful.

[15] Achen and Snidal (fn. 8), 152, n. 24; Philip E. Tetlock, "Testing Deterrence Theory: Some Conceptual and Methodological Issues," *Journal of Social Issues* 43, No. 4 (1987), 85-92.

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More damning still, deductive theories of deterrence, even those built on revised models of net subjective expected utility, do not define their scope conditions. Achen and Snidal extol the virtues of a simple formulation of subjective expected utility: if the expected punishment exceeds the gain, they argue, then opponents will be deterred. They concede that deterrence will fail against sufficiently determined attackers.[16] Recognition that some initiators are not deterrable, while obvious, does little to rescue deterrence theories. A theory worth the name must specify the conditions of its relevance. If it does not, it cannot be tested against any body of evidence, no matter how complete.[17] It also runs the risk of becoming self-confirming because anomalous results can be interpreted after the fact as outside the scope of the theory.[18]

Achen and Snidal's definition of the scope conditions of "rational deterrence theory" is even more confusing than the deceptively simple formulation of the net subjective expected utility of attack suggests. They assert that deterrence theory predicts that deterrence will fail when a deterrer's threat of denial or punishment is absent, incredible, or less valuable than the prize. But it should be obvious that when no threat is made, deterrence is not relevant; these cases lie outside the scope of the theory. The theory is also mute about the conditions in which initiators are likely to judge the prize more valuable than the expected costs of a challenge. It speaks only, and then only in part, to the conditions that make a threat credible.

While the consequences of the failure to formulate scope conditions are serious for the theory of deterrence, their implications are even more troubling for the strategy of deterrence. Leaders can get no guidance on when an attempt at deterrence is appropriate, when it is likely to fail, and when it is likely to provoke an incautious and vulnerable adversary. Be-

[16] Achen and Snidal (fn. 8), 152.

[17] In reviewing the predictive use of subjective expected utility models, Schoemaker (fn. 4, 538-39) argues that evidence counter to the theory can often be refuted because it is indirect. The most irrefutable evidence, which directly concerns the axioms, is discounted in the positivist tradition. Ex post facto interpretation of a decision as rational through the addition of factors not specified by the model treats the optimality of behavior as an essentially unfalsifiable metapostulate. Consequently, ex post empirical models have limited refutation power regarding the corresponding theoretical ex ante model.

[18] Mefford (fn. 7) argues persuasively: "Interpreting the *uninterpreted* key concepts of rational choice requires spelling out for each actor that actor's utility function, perceptions of uncertainty, and perceived set of outcomes. Because there is virtually no limit on the variety of ways that these concepts of rational choice can be mapped onto a political world, rational-choice based models of deterrence seem to suffer from much the same open-ended character . . . of the case-based work. . . . The number of auxiliary propositions that can be attached to the uninterpreted core of rational choice is disturbingly large. It matters which ones are chosen. And, the rational choice theorist's choice of auxiliary propositions is a legitimate target for critical examination" (emphasis in original), p. 9.

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cause deductive theories of deterrence do not specify their scope conditions, leaders are encouraged to inflate the strategy of deterrence to a general strategy of foreign policy.[19]

A better specified theory that solved these problems would no longer be a theory of deterrence. It would have to be based on a much broader set of assumptions about an initiator's calculus and the additional factors likely to influence that calculus. It would incorporate a wider range of strategies appropriate to these assumptions. In some circumstances, strategies of reassurance, which attempt to reduce the incentives initiators have to use force, may be far more effective than deterrence in avoiding war.[20] Insofar as we go outside theories of deterrence, we may build a better theory of strategic choice, but *not* of deterrence, which is limited to the manipulation of credible threat.

The Limits of Rational Choice

Theories of deterrence built on better models of subjective expected utility or game-theoretic analyses of strategic preferences would still suffer from crippling limitations.

Neither theories of deterrence nor rational choice say anything about the all-important preferences that shape leaders' calculations.[21] Achen and Snidal correctly observe that deterrence theory assumes exogenously given preferences and choice options. It begs the question of how preferences are formed. Empirical analyses of decision making suggest that individuals often identify their preferences and options in the course of formulating and reformulating a problem. Herbert Simon has argued:

> To understand political choices, we need to understand where the frame of reference for the actors' thinking comes from—how it is evoked. An important component of the frame of reference is the set of alternatives that are given consideration in the choice process. We need to understand not only how people reason about alternatives, but where the alternatives come from in the first place.[22]

The most important determinant of strategic decisions is not the process of choosing among options but the prior definition and construction of the problem to be decided. Although it is legitimate for theory to set

[19] For a similar argument without reference to the analytical properties of the theory, see Michael MccGwire, "Deterrence: The Problem—Not the Solution," *SAIS Review* 5 (Summer-Fall 1985), 105-24.

[20] For analysis of the interaction between strategies of deterrence and reassurance in avoiding war, see Janice Gross Stein, "Deterrence and Reassurance," in Philip E. Tetlock et al., eds., *Behavior, Society, and Nuclear War* (New York: Oxford University Press, forthcoming).

[21] Jervis (fn. 11, 1989).

[22] Simon (fn. 6), 295.

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its own boundaries, the boundaries set by "rational deterrence theory" exclude most of the theoretically important problems of deterrence and the politically relevant questions leaders confront when they use it as strategy.[23]

Of equal importance, "rational deterrence theory" acknowledges that preferences or estimates change but says nothing about how, when, and why this occurs. The assumption that initiators change their preferences and estimates as a consequence of the actions of the defender lies at the very core of theories of deterrence. Much of the game-theoretic analysis of deterrence posits an ordering of preferences and predicts behavior accordingly. Yet preferences often change through the process of interaction and may do so in ways that are unanticipated by or contradictory to theories of deterrence.

The Cuban missile crisis offers a striking example of this phenomenon. The use of threats by President Kennedy to moderate Soviet behavior unexpectedly increased Soviet estimates of the strategic costs of inaction and shifted the preference ordering of Soviet leaders so as to make a challenge more attractive. Soviet sources report that Kennedy's strategic buildup, his decision to proceed with the deployment of Jupiter missiles in Turkey, and his claims of strategic superiority convinced Khrushchev and Marshal Rodion Malinovsky that it was imperative to do something to improve the Soviet strategic position.[24] Not only did learning occur, but the learning that did occur was contradictory to the predictions of deterrence theories. For both these reasons, the analysis of deterrence in theory and practice is hollow at its center.

A different and more difficult problem for theories of rational choice is the now-well-documented deviations from processes of rational calculation. At times, these can be so severe and pervasive as to defeat deterrence in ways unanticipated by the theory. There is strong evidence from comparative analysis of cases of deterrence failure that initiators frequently distort information about the expected costs of military action, the probability of winning, and the probability that the defender will retaliate—the three variables identified as critical by "rational deterrence theory."[25] Leaders in fact deviate systematically from rational norms in their estimates of the factors specified by "rational deterrence theory" as critical to leaders' calculations.[26] Misperception and miscalculation are

[23] For a similar argument with respect to game theory and the analysis of cooperation, see Robert Jervis, "Realism, Game Theory, and Cooperation," *World Politics* 40 (April 1988), 317-49.

[24] Lebow (fn. 9), passim.

[25] Lebow and Stein (fn. 11), passim.

[26] As Mefford argues (fn. 7), what is at issue is the correspondence between the essential terms in the core of rational models and the auxiliary assumptions of deterrence theory (pp.

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important not only as descriptions of how leaders think, but also as predictors of what leaders choose.

Two kinds of evidence make clear the consequences of omitting the impact of misperception and miscalculation from the analysis of deterrence. The first is drawn from the analysis of a critical case, the second from the results of a deductive analysis premised on "rational deterrence theory." In May 1967, Egypt challenged deterrence because its leaders seriously underestimated the costs of military action and overestimated the probability of winning. They did so even though only a few weeks earlier they had been pessimistic about the probability of winning and skeptical of their military capabilities. The dramatic change in their estimates of relative capabilities cannot be explained by objective changes in the balance of power but rather by motivated distortion in response to pressing political needs.[27]

The "rational theory of deterrence," applied to Egyptian preferences at the beginning of May, would have predicted the success of deterrence. Alternatively, applied to the miscalculated estimates of Egyptian leaders in mid-May, it would have predicted the failure of deterrence, but for reasons the theory does not consider. Egyptian estimates did not change in response to the behavior of the deterrer, but largely in reaction to pressing needs at home and in the Arab world. Theories of deterrence would not have been able to capture or explain, much less predict, the changes in the estimates that determined Egyptian choices. Almost everything relevant, for strategy as well as for theory, is outside their scope.

This fundamental inadequacy of deterrence is acknowledged by Frank Zagare, a leading proponent of game-theoretic analyses.[28] To explain the initial success of the United States in deterring Israel from using military force on May 28, 1967, he explicitly uses Israel's miscalculated estimates of American preferences. In his modeling of the failure of American deterrence only one week later, he works with Israel's accurate estimates of American preferences. Zagare's theory of moves first builds in miscalculation as an exogenous variable to explain the success of deterrence. Its analysis of deterrence failure a week later cannot account for the revision of the estimates that changed the outcome.[29] Again, what is

7-10). These auxiliary assumptions are the costs of war, the probability of winning, and the probability of retaliation. Achen and Snidal ignore the necessity to test these propositions as the core of an empirical theory of deterrence.

[27] Stein (fn. 11).

[28] Zagare (fn. 2, 1987), 1-7.

[29] Technically, Zagare's analysis is based on a different interpretation of the meaning of two-by-two games. The numbers denote rates of payoff rather than single payoffs, with players moving around within the matrix.

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critically relevant to theory and strategy is outside the model. A recent review of game-theoretic analyses of deterrence acknowledges this critical weakness and urges that rational actions be treated in part as a function of rational beliefs.[30]

"Rational deterrence theory" cannot by definition accommodate systematic deviations from rational choice. Deterrence theorists are trying to cope with this dilemma by utilizing recent advances in game theory that incorporate uncertainty and sequential equilibria into their models.[31] But these models do not address the problems of misperception and miscalculation that we have identified. Uncertainty is the result of inadequate information. Misperception and miscalculation arise from faulty evaluation of available information. Studies of deterrence and intelligence failures find that error rarely results from inadequate information but is almost always due to theory and stress driven interpretations of evidence.[32] On this one, "rational deterrence theorists" are barking up the wrong decision tree.

Achen and Snidal assert that psychopathological reasoning lies outside the scope of rational deterrence theory. But they and others fail to define the boundaries of such reasoning.[33] More generally, deterrence theorists have failed to delineate the boundaries of rational choice: if the problem of deterrence theory is defined as whether or not a rational actor will choose to attack despite a credible threat of retaliation, then specifying the

[30] Barry O'Neill, "Game Theory and the Study of the Deterrence of War," in Paul C. Stern et al., eds., *Perspectives on Deterrence* (New York: Oxford University Press, 1989). O'Neill notes: "Previous concepts had tried to draw a sharp line between rational and irrational actions based solely on the utilities of the outcomes, but recent approaches regard rational actions as interlocked with rational beliefs. . . . Whether they [the players] adopt these beliefs is an empirical question, and thus the validity of an equilibrium becomes a question of social science rather than mathematical axioms. Models like this seem more realistic and reduce the isolation of game theory applications from other social sciences."

[31] See, for example, Robert Powell, "Crisis Bargaining, Escalation, and MAD," *American Political Science Review* 81 (September 1987), 717-36, and "Nuclear Brinkmanship with Two-Sided Incomplete Information," *American Political Science Review* 82 (March 1988), 155-78; R. Harrison Wagner, "Reputation and the Credibility of Military Threats: Rational Choice vs. Psychology," paper presented to the Annual Meeting of the American Political Science Association, September 1988.

[32] Harvey de Weerd, "Strategic Surprise and the Korean War," *Orbis* 6 (Fall 1962), 435-52; Alexander L. George and Richard Smoke, *Deterrence in American Foreign Policy: Theory and Practice* (New York: Columbia University Press, 1974), 184-234; Lebow (fn. 11, 1981) passim; Janice Gross Stein, "Calculation, Miscalculation, and Conventional Deterrence II: The View from Jerusalem," and Richard Ned Lebow, "Miscalculation in the South Atlantic: The Origins of the Falkland War," in Jervis, Lebow, and Stein (fn. 9), 60-88, 89-124.

[33] The Egyptian Minister of War, Shams al-Din Badran, estimated in May 1967 that Egyptian forces could handle military intervention on behalf of Israel by the Sixth Fleet; cited by Mahmoud Riad, *The Struggle for Peace in the Middle East* (London: Quartet Books, 1981), 23. Is this, in Snidal and Achen's terms, psychopathological reasoning that falls outside the scope of a "rational theory of deterrence," or evidence of systematic miscalculation that defeats the expectations of rational deterrence theory?

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criteria of rationality is absolutely critical to determining the scope of the theory.

Are deductive theories of deterrence restricted to cases where leaders rationally calculate subjective expected utility or reasonably approximate these norms? How much do leaders have to deviate from norms of rationality to be considered nonrational? When initiators systematically discount the likely costs of military action because the choices they confront are so painful, or dismiss clear evidence of the likelihood of retaliation by a defender, or ignore the central trade-offs among alternatives, are they acting outside the scope of "rational deterrence theory"? If they are, then "rational deterrence theory" is restricted to a small proportion of the cases where deterrence is at issue. A general review of subjective expected utility theory concludes that it fails as a predictive and descriptive model in almost all the cases where it has been empirically tested. This is due to its inadequate specification of psychological principles of judgment and choice.[34] Rational deterrence theory may be elegant but irrelevant.

This problem is not unique to deterrence theory but applies to rational-choice theories generally. Rational-choice theorists confront an awkward dilemma. They can pursue elegance at the price of relevance, or they can reject flawed assumptions and reformulate their theories. Paul Schoemaker complains that they have chosen the former by trying to save their theories by tinkering with one or two peripheral assumptions. "Having invested heavily in complex deductive structures, with wide domains of applicability and mathematically elegant decision models that allow for easy aggregation across people, it is a natural tendency to patch up the theory cosmetically. A better alternative," he insists and we agree, would be "to examine closely the type of anomalies reported and the cognitive reasons underlying them."[35]

Testing Theories of Deterrence

Achen and Snidal do not dispute the evidence of systematic deviations from rational choice identified in the comparative analysis of cases of deterrence failure. They defend "rational deterrence theory" with the assertion that its purpose is not to model leaders' calculations but to predict outcomes. They insist that if deterrence theory makes accurate predictions, evidence of constraints to rationality is of no concern.

Leaving aside for the moment the claim that good theory can be built

[34] Schoemaker (fn. 4), 548.

[35] *Ibid.*, 554.

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on flawed behavioral assumptions, there is no justification for the assertion that deterrence theory has predictive value. The predictive capacity of rational deterrence theory has never been demonstrated nor satisfactorily tested. What evidence there is, indicates that it is a poor predictor of behavior in critical cases.

How has deterrence theory been tested empirically? Some scholars have used aggregate data analysis to test the predictive power of the theory in cases of general deterrence.[36] In a study of extended general deterrence cited approvingly by Achen and Snidal, Erich Weede identifies all logically possible cases of deterrence, without examining whether the criteria of deterrence are present, and then finds significant differences in the incidence of war.[37] The study fails to define the scope conditions of the theory and does not investigate empirically whether or not deterrence was relevant to the thousands of cases it identifies. By the nature of his case selection, many if not most of his cases, we suspect, fall outside the scope of deterrence. If in these cases neither party contemplated a challenge nor sought to deter one, the study dramatically overestimates the success of deterrence.

Paul Huth and Bruce Russett are more rigorous in defining what constitutes an immediate deterrence encounter and have identified sixty cases of extended immediate deterrence in this century.[38] Under the most permissive criterion of success, deterrence succeeds only 59 percent of the time.

To determine the causes of success and failure, Huth and Russett use indirect measures of alliance commitments, balance-of-power ratios, and past bargaining behavior to tap indirectly leaders' estimates of commitment, capability, and credibility. They then use probit analysis to identify the structural relationships between these variables and the outcome of deterrence. Their method allows them to estimate the relationship between structural and behavioral conditions and the outcome of deterrence but not to test directly the propositions deduced from deterrence theory. They nevertheless acknowledge the importance of directly tracing the impact of leaders' estimates on the outcome of deterrence.[39]

[36] For the distinction between "immediate" and "general" deterrence, see Patrick Morgan, *Deterrence: A Conceptual Analysis*, rev. ed. (Beverly Hills: Sage Publications, 1983).

[37] Weede, "Extended Deterrence by Superpower Alliance," *Journal of Conflict Resolution* 27 (June 1983), 231-54.

[38] Huth and Russett, "What Makes Deterrence Work? Cases from 1900 to 1980," *World Politics* 36 (July 1984), 496-526, and "Deterrence Failure and Crisis Escalation," *International Studies Quarterly* 32, No. 1 (1988), 29-46; Paul Huth, "Extended Deterrence and the Outbreak of War," *American Political Science Review* 82 (June 1988), 423-44, and *Extended Deterrence and the Prevention of War* (New Haven: Yale University Press, 1988).

[39] Russett (fn. 9), and Huth (fn. 38).

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Our empirical research has concentrated on immediate deterrence failures. We chose to work with failures because they are more easily identifiable through the observable behavior that occurs. Achen and Snidal question whether this approach can test deterrence theory. They claim that it can only identify and describe failures of deterrence as a strategy. They are wrong. We define the scope conditions of our cases to conform to the expectations of theories of deterrence and are therefore able to identify failures of theory as well as of strategy.

We distinguish between two kinds of deterrence failure: those that present no problem to deterrence theory and those that do. The former consists of cases where deterrence failed but where the strategy was implemented poorly or improperly. We draw no inferences about deterrence theory from these cases. The latter set of cases have damaging implications for the theory. In all of them, the defender met the conditions of deterrence theory: the deterrer carefully defined the unacceptable behavior, threatened retaliation, had the capability to implement the threat, demonstrated resolve, but the challenger still proceeded to use force. In some of these cases, the contradiction between theory and behavior can be attributed to the flawed calculations of the challenger. Our empirical evidence indicates that this is a widespread phenomenon. A second kind of failure occurs when challengers calculate as deterrence theory expects but behave contrary to its predictions. They do so because their calculations are determined by considerations outside the realm of deterrence theory. Cases of this kind stand as a stark challenge to that theory.

Ours is a highly restrictive definition of failure, for it deliberately excludes cases where challengers resorted to force but defenders did not attempt to deter or did so ineptly. Unlike Achen and Snidal, we do not consider that deterrence has failed when the threat of denial or punishment is absent.[40] Because we base our analysis only on cases that meet the conditions of deterrence theory, we can claim to test for failures of that theory.

We do not contend that our analysis of immediate deterrence failures encompasses the universe of cases, either of deterrence failures or successes. This is in large part because of the data requirements of the analysis of deterrence. To identify cases, we need valid evidence of the calculations of both the initiator and the deterrer. We seek, whenever possible, not only the reconstructions of the participants after the fact, for, as Achen and Snidal note and we concur, this kind of evidence is subject to well-known biases. We look for convergent evidence from several par-

[40] Achen and Snidal (fn. 8), 152.

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ticipants from each side, and for historical documentation as well. We remain unpersuaded that indirect measures used in the aggregate analysis of deterrence are valid measures of the variables specified by theories of deterrence.[41] These criteria of reliability and validity of evidence sharply limit the number of cases suitable for analysis. Fortunately, for many of the critical cases of deterrence failure, better evidence on the calculations of both sides is becoming available.

Detailed analysis of cases reveals a problem that is ignored by deterrence theory. This is the difficulty of distinguishing challenger from defender in any given case.[42] Deterrence theories assume that there is a clear analytical distinction between challenger and defender that is obvious not only to scholars but to the actors themselves. In reality, this distinction is generally blurred. Both parties to a deterrence encounter often consider themselves to be the defender. This is not just a technical problem, but one with significant theoretical consequences. Leaders' conceptions of themselves as initiators or defenders have important consequences for their behavior. It also makes coding of deterrence cases very difficult and at times arbitrary. The 1914 crisis is an obvious case in point. So too are the Cuban missile crisis and Egypt and Israel in 1967.

More important, even if we were able to identify the universe of cases and determine who was the challenger and who the defender, establishing the proportional rate of deterrence success and failure is not the only important question. Even a single deterrence failure can lead to war, perhaps nuclear war. The absolute number of cases of deterrence failure is, in itself, alarming. We regard it as intrinsically important to explain—and ultimately predict—cases of deterrence failure, irrespective of the general performance of the theory and the strategy. We contend that the essential first step is to trace the processes of deterrence failure in "critical" cases.[43]

Our comparative analysis of critical cases of deterrence failure was designed to identify the conditions under which deterrence is likely to fail.

[41] Achen and Snidal cite Ragin's persuasive argument that contextual and holistic social and political forces are frequently blurred and averaged in a meaningless way in "variable-oriented" approaches. Ragin, as does George, urges the use of case studies to trace detailed causal sequences in different contexts. Charles C. Ragin, *The Comparative Method* (Berkeley: University of California Press, 1987).

[42] Edward A. Kolodziej, "The Limits of Deterrence Theory," *Journal of Social Issues* 43, No. 4 (1987), 123-34, makes this point in the context of general deterrence.

[43] See Harry Eckstein, "Case Study and Theory in Political Science," in Fred I. Greenstein and Nelson W. Polsby, eds., *Handbook of Political Science*, Vol. 7 (Reading, MA: Addison-Wesley, 1975), 79-138, and Arend Lijphart, "Comparative Politics and the Comparative Method," *American Political Science Review* 65 (September 1971), 682-93, and "The Comparable Case-Strategy in Comparative Research," *Comparative Political Studies* 8 (July 1975), 158-177, for a discussion of the importance of the analysis of "critical" cases.

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In the analysis of these cases, we looked carefully at the relationships among variables specified by "rational deterrence theory."[44] We attempted to assess how and why differences in these relationships produced unanticipated outcomes and then to identify variables omitted by the theory and trace their interaction through the process of deterrence failure.[45] The empirical analysis of cases of deterrence failure consequently served two functions. It provided a partial test of "rational deterrence theory" insofar as it examined the interaction among variables the theory specifies. It also identified additional variables and their interrelationships that, our analysis demonstrated, have an important impact on the outcome of deterrence.

We explicitly acknowledge that our analyses provide only a partial test of theories of deterrence. Our analysis of deterrence failure can only be tentative, since some of the relationships among variables that appear to explain failure may also be at work when deterrence succeeds. Contingent propositions derived from a controlled comparison of deterrence failures must be validated against cases of deterrence success.

Deterrence succeeds when a deterrer explicitly attempts to prevent a use or deployment of military force and a would-be challenger considers initiating that action but is dissuaded by the defender's threat. These criteria of selection, we recognize, are likely to reduce the number of cases of immediate deterrence success we can identify. Since there are often no direct behavioral traces of success other than a defender's threat of retaliation, a search of the historical record is open-ended. Moreover, leaders are less likely to write and to speak, even privately, about an adversary's success in deterring them from a challenge. Identifying cases of general deterrence success over time is even more difficult. If deterrence succeeds over time, would-be challengers may never consider a challenge and defenders may never need to threaten explicitly or to manipulate the risk of war. In the absence of evidence, identification of the success of deterrence depends entirely on counterfactual argument. Achen and Snidal resort to precisely this kind of argument in their discussion of wars that did not occur.

Sensitive to the data requirements and to the threats to valid inference that arise from counterfactual argument, we are currently engaged in the identification of critical cases of immediate deterrence success. In all of

[44] See Lebow and Stein (fn. 11).

[45] For a discussion of the advantages of "process tracing" in critical cases, see Alexander L. George, "Case Studies and Theory Development," paper presented to the Second Annual Symposium on Information Processing in Organizations, Carnegie-Mellon University, Pittsburgh, PA, October 15-16, 1982.

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these cases, we insist on convergent streams of evidence indicating that would-be initiators contemplated challenges but were dissuaded by deterrent threats. Evidence of the initiator's intentions and calculations is essential to determine whether the failure to challenge was the consequence of deterrence or of other considerations and constraints.

Once we have identified the critical cases for analysis, we intend to examine the propositions deduced from "rational deterrence theory" as well as those derived from the analysis of cases of deterrence failure. This comparative analysis will provide a more valid test both of existing theories of deterrence and the contingent propositions that have been identified through the empirical analysis of deterrence failure.

We contend that "rational deterrence theory" has not yet been tested satisfactorily. The first step has been taken. Analysis of deterrence failures has identified important variables omitted by the theory and traced their impact through to the failure of deterrence. Our analysis suggests that many, perhaps most, challengers are driven less by opportunity than by vulnerabilities and needs that motivate errors in their estimates of the variables specified by rational theories of deterrence as critical to the success of deterrence.

Comparative analysis of deterrence success is the essential next step if we are to assess the predictive power of "rational deterrence theory" as well as the validity of the contingent propositions derived from our analysis of deterrence failure. This analysis must be done with evidence from cases appropriate to the specifications of theory. With tongue only mildly in cheek, we might add that the responsibility for rigorous testing lies not only with the critics but also with the proponents of "rational deterrence theory." It is they, after all, who assert its predictive power as an empirical theory of strategic behavior. The challenge is not to assert but to demonstrate.

Why Deterrence Is an Inadequate Theory

Our analysis has highlighted some of the more important conceptual failings of deterrence theory. We have argued that deterrence theories presuppose that leaders are (1) instrumentally rational, (2) risk-prone gain maximizers, (3) free of domestic constraints, and (4) able correctly to identify themselves as defenders or challengers. All of these core assumptions are unrealistic and contradicted by empirical evidence.

Achen and Snidal contend that this is not a problem for deterrence theory. They assert that the only criterion of good theory is its predictive capacity. They further contend that a theory based on unrealistic assump-

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tions can still be a good predictor of behavior.[46] Philosophers of science challenge this assertion.[47] Although they disagree among themselves about the nature of social science theory and whether or not it can or ought to be scientific, they categorically reject the notion that good predictive theories can be built on unrealistic assumptions.

Theoretical models often contain two types of terms: those referring to unobserved but actual phenomena, and those referring to ideal entities. Neutrons, quarks, and black holes are examples of the former. They can be measured, counted, and detected through their interactions, and their existence and properties thereby confirmed or disconfirmed. Idealizations, by contrast, are derivative concepts that are not applicable to any empirical situation. Neither an ideal gas nor a perfect vacuum can be found in nature. They are concepts derived from other theoretical assumptions, fundamental to the model, that do have empirical referents. They serve an important function in theory: they provide extreme values against which empirical observations can be interpreted.

The problem with rational models is not that they contain idealizations but that these idealizations are their fundamental assumptions.[48] Such assumptions as the rational decision maker, perfect information, and a politically neutral environment are idealizations that lack any empirical referent. Rational deterrence theories are accordingly "theories" about nonexistent decision makers operating in nonexistent environments. If rational deterrence theories are to be useful as empirical theories, they must open their fundamental assumptions and behavioral implications to empirical evaluation, not protect or shield them from scrutiny.[49]

[46] This is a line of argument that originated with Milton Friedman, "The Methodology of Positive Economics," in Friedman, *Essays in Positive Economics* (Chicago: University of Chicago Press, 1953), 3-43. It entered political science through the work of Anthony Downs, *An Economic Theory of Democracy* (New York: Harper & Row, 1957).

[47] For an excellent discussion of the scientific problems of rational models, see Terry M. Moe, "On the Scientific Status of Rational Models," *Journal of Political Science* 23 (February 1979), 215-43.

[48] *Ibid.*, 226.

[49] Moe makes this argument for rational models in general (*ibid.*, 235).

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THE RATIONAL DETERRENCE DEBATE

By GEORGE W. DOWNS*

Introduction

THE defense of rational deterrence theory by Christopher Achen and Duncan Snidal and the comments by Alexander George and Richard Smoke, Robert Jervis, and Richard Ned Lebow and Janice Stein delineate two archetypal approaches to the study of deterrence and the reasons why the proponents of each believe their approach to be superior. While all of the authors offer general statements on the merits of the rival approach, they provide few specific examples of how the vision of deterrence that they support has been shaped by the insights of the contending school. Ultimately, Achen and Snidal argue for reliance on the axiomatic methods of microeconomics, and the commentators defend the inductive experimental and quasi-experimental methods of psychology and comparative politics. Since the debate has had the inevitable consequence of emphasizing the differences between the archetypes, this essay will focus on problems that they share and the important ways in which they complement each other.

No doubt there are times when the desire to maintain the integrity of the two archetypes keeps scholars from succumbing to a soggy ecumenicalism in which depth of analysis is replaced by shallow consensus; however, this desire can also delay the acceptance of an insight because it has emerged from the rival, and it can deflect attention from problems that are shared by both. The chances that this will be true are increased greatly when, as is the case here, the sides themselves are motivated by two quite different academic disciplines (psychology and economics) that employ markedly different methodologies.

In the present debate, the interrelationship between the two positions is understated. It will be argued here that rational deterrence theory has already evolved in directions that proponents of the case-study archetype consider desirable, and that more of the same is needed. Moreover, rational deterrence theorists have sometimes used a methodology to refine

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their theory which resembles that of their rivals (e.g., the use of nonrandom samples subject to selection effects). On the other hand, the proponents of case studies evaluate the effects of psychological and domestic political biases on deterrence by using implicit conceptions of benefit and cost that play a central role in rational deterrence theory. Unless these analysts are also willing to exploit some axiomatic techniques to trace through the implications of their inductive research, it seems probable that their theories will remain fragmented and the practical implications of their findings will remain unclear. This is not to suggest that the complementarity of the two approaches demands that they be fully integrated into a single synthetic approach. Their symbiosis can also be achieved independently, in stages. For example, we will see that, if those who operate from a psychological perspective are successful in convincing foreign policy decision makers to reduce biases and distortions, they will not only have made the world safer; they will have made rational deterrence theory more relevant.

Revising Axiomatic Models of Deterrence: Induction and Assumption

One of the difficulties in evaluating the arguments of proponents and critics of rational deterrence theory is the absence of a common vision of what "it" is. Achen and Snidal are correct in stating that the myriad versions of these theories have certain properties in common; but to say that they all share a concern for the fundamental deterrence problem (i.e., the use of threats to induce an opponent to act in desirable ways) leaves the door open for a wide variety of specifications. In particular, it becomes possible for both the "weak" and "strong" versions (as they will be called) of the rational deterrence argument to be treated as representative of the species. The weak version simply views the outcome of choices as a function of expected benefits and costs; this result is bound to emerge in even the most committed case-study advocate's analysis of deterrence or any other volitional behavior. Until physiological psychologists convince us that the cognitive component of such choices is either trivial or completely determined by hormone levels, there is little alternative. If expectations about benefits and costs do not shape behavior, what does? In conjunction with the papers that respond to Achen and Snidal, it will be argued that the price of abandoning the loose organizing structure of the weak version of deterrence is enormous.

The "weak" version of the rational deterrence model is of limited value as a guide to policy, however, for precisely the same reason that

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Achen and Snidal find inductive theory building to be of limited value: its absence of logical constraints leads to a theory that lacks fecundity. While it may not be possible to be "too rich or too thin," parsimony can be carried to excess. Science—especially biology—is replete with theories whose richness is a function of their considerable complexity. In this case, prediction requires the generation of "strong" models that specify benefits and costs, the shape of utility functions, the size of the stochastic component associated with assessments of the cost of war and the probability of winning, the extent of problems of misperception and control, and so forth. Two synthesic questions that do not get much treatment in Achen and Snidal's essay are: (1) How many such complicating assumptions need to be added to the weak version of the rational deterrence model before it becomes a strong model that is a source of theoretical fertility and policy inspiration? (2) Where are these complications going to come from?

Before trying to argue that the case-study proponents can be seen as providing an invaluable source for creating a strong version of the deterrence model, let us consider why these questions are not more frequently addressed in the formal modeling literature. One disturbing possibility lies at the intersection of the nonfalsifiable character of the weak model and the difficulty of testing any proposition about the nature of deterrence empirically. The slipperiness of subjective utility models applied to deterrence is notorious. If a threat (or level of capability and the presumed ability to employ it) succeeds, rational deterrence has scored a success. If it fails, it can almost always be argued that the threat was too small, not taken seriously, badly communicated, or that it represented a cost that was less than a cost expected to be borne in the future (e.g., the Japanese decision to attack Pearl Harbor). If evidence in the memoirs of decision makers suggests that their decisions were motivated by irrational calculation, a proponent of rational deterrence theory can contend that they were either misrepresenting their actual thoughts or were impelled by elemental forces that made their thoughts irrelevant. That such defenses may be correct in any given case is beside the point; what matters is that history rarely presents evidence that unambiguously falsifies the weak version of rational deterrence theory. A (post facto) batting average of one thousand is not much of an inspiration for change.

Suppose that our rational deterrence theorist is well aware of the shortcomings of the weak model—as Achen and Snidal certainly are—and constructs a strong version capable of generating predictions on the basis of a well-defined information set. Aside from the fact that there are countless possible models and little theory that tells us what to include in

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the information set, the analyst still faces the task of assessing the model's accuracy. Achen and Snidal describe how difficult this will be because of problems in defining the relevant population from which to draw a sample. If war breaks out, we have some confidence (but no certainty) that there has been a case of attempted deterrence. Whatever the virtues of a sample made up of such cases, a selection effect is at work here that excludes deterrence successes. The sample might be improved somewhat by including serious crises that did not result in war, but there is still potential selection bias. What about "crises" that never happened because of successful deterrence, or because both sides were satisfied with the status quo? And that is only the beginning. What about situations in which neither a crisis nor a war occurred because one side chose to capitulate to the desires of the other? What should our time frame be? Has deterrence failed (or succeeded) if war comes in three years, but not in two? Is every day without war a success? The difficulty of finding defensible answers to these sorts of questions explains why most political scientists would rather do formal modeling (or case studies). More to the point, it supplies the creator of even a strong rational deterrence model with good arguments to the effect that a true sample would treat his or her model more kindly than does any given data set.

It should be noted that this warning on the perils of selection bias may be too broadly stated. The problem hinges on the distinction between deterrence failure and failures of deterrence theory. Even if, as Achen and Snidal point out, the case-study literature is biased toward inclusion of deterrence failures, it may still provide a basis for testing deterrence theories. It is possible to picture a 2×2 contingency table in which the columns refer to the outcomes of the application of deterrence (i.e., peace and war) while the rows refer to the prediction of peace and war generated by rational deterrence theory. As Achen and Snidal observe, and as others like Lebow and Stein fully realize, these are not the same thing. An outcome of war is not, by itself, inconsistent with rational deterrence theory. It is expected that the theory will sometimes predict that war will take place. What Achen and Snidal do not point out is that there is no special problem in oversampling from a population of deterrence outcome failures unless the rows and columns are correlated—that is, unless the outcome failures were selected on the basis of theory failures. While it is undeniable that outcome failures have been overrepresented in the case-study literature, it is less clear that theory failures have been correspondingly oversampled, since there is rarely a strong theory available to work with. A priori, we have no reason to believe that failures of deterrence threats should be correlated with failures of deterrence theory. As

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a consequence, the possible bias in the case-study literature toward inclusion of deterrence failures does not necessarily mean that the case-study literature (in the aggregate) cannot provide tests of deterrence theory.[1]

Regardless of whether evidence from case studies is less biased than one might assume it to be, the difficulties of testing even a strong theory of deterrence and the slippery virtues of the weak theory do not create an atmosphere that is receptive to suggestions for change. Why fix something that is not broken?

Yet deterrence models do evolve. Theorists who are not seduced by the magical virtues of the weak model create strong models that vary across individuals and over time. What is the inspiration for these modifications? One possibility is that they are inspired in part by something closely akin to case studies. That is, the rational deterrence theorist observes a nonrandom sample of deterrence situations and adopts a new assumption that many (including the theorist when conversing with a nonpsychologist) would characterize as more "realistic." The treatment of uncertainty and misperception is a good example. Ten years ago, the average formal modeler spent little time examining the consequences of these factors for deterrence strategies. Now rational deterrence theorists are dealing with them—even to the point of recognizing utility uncertainty.[2] Regardless of the source of such changes, it is impossible to read the work of George and Smoke, Lebow and Stein, or Jervis, and avoid the conclusion that such changes were indeed necessary.

The inclusion of uncertainty in deterrence models on the basis of case-study information is not something distinct from theory building. It *is* theory building. It constitutes a respecification of the theory that would be no less important than dropping the assumption of the unitary actor. Unless one is willing to argue that the rules of logic or mathematics constitute the essence of every theory—an extreme position that would imply that the theories of superconductivity and relativity were either equivalent or different only with respect to the mathematics that they employ—the heart of a theory is defined both by its basic elements (often variables) and by the ways in which they are presumed to interrelate. When a large number of changes are made in either, or when those that are made have important consequences, it is common to describe the result as a new theory.

The point here is that the specification of any strong model of rational

[1] The author thanks David Rocke for offering this observation.

[2] See Robert Powell, "Nuclear Deterrence and the Strategy of Limited Retaliation," paper presented at the convention of the American Political Science Association, Washington, DC, September 1988.

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deterrence, and consequently the generation of theory, is likely to stem from case-study information or its informally acquired equivalent. No data—much less data derived from a random sample—have been used to justify the recent preoccupation with uncertainty; it is doubtful whether subsequent refinements to the rational theory of deterrence will be so justified. Given the sampling problems that have been described, there is nothing wrong with proceeding in this fashion. The difficulty is that this is the sort of behavior for which case-study advocates are condemned. Even worse, one suspects that the incorporation of uncertainty is inspired not just by knowledge of the choices that decision makers have made, but by the character of the mental calculations that they say lie behind their decisions. Indeed, it is not clear how uncertainty can be estimated in most strategic contexts without reference to such mental calculations. Yet Achen and Snidal go to some lengths to tell us that rational choice theory has no interest in such things.

The case-study researcher might well feel that a double standard is operating here. When a devotee of rational models takes steps to make the assumptions more realistic (and the model more fertile and accurate in its predictions) by incorporating uncertainty, he or she is not permitting theory to emerge from "facts and low-level generalizations," but engaging in an undefined act that is identical to that used by every successful scientist from Newton and Darwin to Gerald Edelman and Stephen Hawking.[3] When a political scientist with a psychological preoccupation argues, on the basis of case studies and experimental data, that the rational deterrence model is flawed by unrealistic assumptions, he or she is chastised.

Moreover, the portion of rational deterrence theory that is derived inductively from nonrandom samples will probably increase, at least in the near future. In the absence of perfect information, beliefs are important components of any equilibrium. For example, sequential equilibrium models, which (for good reasons) are now the rage among rational deterrence theorists, require that each side possess a strategy and a belief system. The character of belief systems and the algorithms used to update them are currently being specified in very arbitrary ways. Often, the modeler assumes that information about the opponent's capability and strategy is updated by a Bayesian algorithm. If the prior probabilities and likelihoods are correctly specified, this procedure is quite sensible. But where does this specification come from? The initial specification of the belief system is often limited to two or three arbitrary discrete elements that represent mathematically tractable ideal types. A more realistic spec-

[3] See Achen and Snidal, "Rational Deterrence Theory and Comparative Case Studies," *World Politics* 41 (January 1989), n. 51.

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ification that involves a multidimensional, continuous parameter space will eventually have to be devised. Psychologists and case-study researchers are a logical source of inspiration.

The theorist also has to consider the possibility that one side will engage in an act to which the other side has assigned a prior probability of zero because this possibility cannot be handled by Bayes' rule. Kreps and Wilson make an assumption about how this problem would be solved, but they would be the first to acknowledge that there is no particular justification for it.[4] Even if zero-probability events could be eliminated by using a continuous parameter space and/or choice space, the occurrence of a very rare event might well lead one to reevaluate the prior belief system that produced this prediction instead of updating according to Bayes' rule. It is, of course, possible to specify a metabelief system to guide this reevaluation, but this increased complexity makes model specification even more problematic.

At this point in the development of formal models of deterrence, assumptions of convenience are more than justified. We have to walk before we can run. But there will come a time when it will make sense to refine these assumptions just as the perfect information assumption was refined. When that time arrives, these changes can come from a rational theorist reflecting on how he would update his belief system or from the work of cognitive psychologists and those who have looked at a wide variety (but still a nonrandom sample) of deterrence cases. The latter source should be taken very seriously.

This is not to argue (as case-study advocates sometimes do) that formal theorists must incorporate every newly discovered heuristic or bias into their models. Few of these heuristics and biases are universal. They are employed to different extents by different individuals and sometimes vary depending on the salience of the decision in question. It is also likely that governments are able—or can acquire the ability—to design mechanisms to compensate for some of their perverse effects. On the other hand, to dismiss the enormous experimental literature that shows that individuals frequently make choices (not simply mental calculations) which are at odds with the predictions that would be generated by a subjective utility model seems indicative of archetype hubris. The outcome of tennis matches and the patterns of her shots may show that Steffi Graf plays "as if" she did rapid computations in Newtonian physics and game theory. Unfortunately, an experimental literature in psychology that has no equivalent in rational deterrence theory shows that all too often people

[4] David M. Kreps and Robert Wilson, "Sequential Equilibria," *Econometrica* 50 (July 1982), 863-94.

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do not make choices or behave in accordance with either Newtonian physics or game theory.

Amos Tversky and his colleagues did not bring about a revolution in cognitive psychology by reading the memoirs of their subjects or by asking them why they made the choices they did.[5] They examined choices in excruciating detail. It is this now vast experimental literature that plays a key role in shaping the work of the case-study proponents. The results, however, are not as clean as the psychologists sometimes imply. It is as if Steffi Graf, the decision maker, seems to behave like a physicist and game theorist on some days, at some temperatures, and in some cities—and like a bewildered rock guitar player on other occasions. These sorts of findings are messy, but they are not naive. Collectively, they are the justification for the psychologists' growing insistence that the burden of proof is on the rational actor modelers; they must convince readers that their axioms are a reasonable basis for theory generation. Rational deterrence theorists, like their colleagues in microeconomics, have some thoughtful justifications for their assumptions, but case-study advocates and other critics will not be placated by arguments based on the virtues of parsimony and the difference between choice behavior and mental calculations.

In sum, although not all psychological or case-study research can or should be integrated into axiomatic deterrence theory, formal modelers should consider its implications seriously. The centrality of uncertainty and the character of sequential equilibrium models suggest that the minimal psychological content that Achen and Snidal believe is associated with formal modeling in this area may have to be expanded. The models and the policy prescriptions that result will be better for it.

Assessing Implications of Case Studies: Deduction and Prescription[6]

Like their rational deterrence counterparts, advocates of the psychology/case-study archetype provide a rich and informative critique of the

[5] See Daniel Kahneman, Paul Slovic, and Amos Tversky, *Judgement under Uncertainty: Heuristics and Biases* (Cambridge: Cambridge University Press, 1982).

[6] Although the commentaries focus on psychological sources of bias, there are those who argue that the non-unitary character of the nation-state can also produce systematic biases. See Robert D. Putnam, "Diplomacy and Domestic Politics: The Logic of Two-Level Games," *International Organization* 42 (Summer 1988), 427-60; Alexander L. George, Phillip Farley, and Alexander Dallin, eds., *U.S.-Soviet Security Cooperation: Achievements, Failures, and Lessons* (New York: Oxford University Press, 1988); and Kenneth A. Oye, "Underprovision of Compensation: Implications of Imperfect Information and Fragmented Actors," paper presented at the convention of the American Political Science Association, Washington, DC, Sep-

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opposition. The authors represented here note the tautology and verification problems that plague the weak model, as well as the scarcity and the arbitrary specification of strong models. They are particularly helpful in describing how many auxiliary assumptions and how much information must be added to a weak rational deterrence model before it can generate useful predictions. They are also correct in arguing that many policy decisions hinge more on "diagnostics"—knowing what benefits and what costs to include—than on any complex deduction. Still, with the exception of Robert Jervis, they do not stress the interrelationship of the two approaches any more than their rational deterrence counterparts do.

Part of the problem is that the weaknesses of the rational deterrence theory approach sometimes lead Lebow and Stein as well as George and Smoke close to suggesting that the expected utility approach and its deductive apparatus should be jettisoned wholesale. That is probably impossible, and almost certainly undesirable, for at least two reasons. First, as we have already noted, the basic framework of rational deterrence theory provides a useful organizing structure for which no alternative exists. One doesn't have to be an economist or to accept any extant formulation of the strong model to recognize the value of speaking in terms of expected benefits and costs. Expectations about benefits and costs (pleasure and pain, good and bad) are as much a part of the psychology of decision making as they are of the economics of decision making. Indeed, it is probably for this reason rather than because of anything connected with the assumption of a unitary decision maker that Achen and Snidal were able to discover such language in the George and Smoke volume. The argument is not whether benefits and costs are useful summary terms, but over other issues: what they encompass, how frequently they are redefined, and how they are estimated. If the case-study school tries to expunge every mention of benefit and cost as indicative of collaboration with the enemy, it will lose its sense of realism and generate dozens of disconnected hypotheses that are collectively inscrutable.

A second reason to avoid the wholesale renunciation of everything connected with the rational deterrence theory is that it leads one to underestimate the critical importance of deduction in theory building and policy design. Lebow and Stein appreciate better than their more formal counterparts that good policies and good predictions will require the estimation of misperception and risk acceptance, but they do not tell us how we can translate these estimates into policy decisions or theory. Instead,

tember 1988. For a contrary position, see Christopher H. Achen, "When Is a State with Bureaucratic Politics Representable as a Unitary Rational Actor?" paper presented at the convention of the American Political Science Association, Washington, DC, September 1988.

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like George and Smoke, they promote the generation of necessary and/or sufficient conditions and contingent but not probabilistic predictions. The relationship of these products to the evolution of a theory of deterrence or other complex behavior is unclear. Few practicing psychologists, for example, would feel confident in enunciating a sufficient (much less a necessary and sufficient) condition for divorce or shoplifting. Any variable associated with either behavior is always associated with instances of its absence. Many unhappy, miserable, and abused people remain married; many impoverished and aggressive people refrain from shoplifting. If the antecedents of decision makers' behavior are too numerous, subtle, and complex to be captured by a subjective utility model, are they likely to be illuminated by a category that is almost never employed to explain individual behavior?

The search for necessary conditions is problematic because the utility of a necessary condition is contingent and poorly understood. There are an infinite number of necessary conditions for any phenomenon. For example, it is true that all armies require water and gravity to operate, but the contribution of such universals is modest. Even the weak rational deterrence model, whatever its numerous faults, tells us that the search for meaningful necessary conditions is likely to be futile unless there are very few factors that enter a decision maker's benefit/cost calculus. That is true because, ceteris paribus, the more factors there are, the more likely it is that the absence of one factor can be compensated for by the addition of another.

In order to use deduction, one need not assume preference stability or believe that there is a clear analytical distinction between challenger and defender. Deduction can be as handy in exploring the implications of algorithmic and heuristic complexity when two actors interact in a deterrence situation as it is in discovering an economic equilibrium. Indeed, its use is unavoidable. Case studies and inference may tell us Hitler's priorities, resources, expected costs, and propensity to take risks, but deduction will still be needed to predict his response to the strategic choices available to another head of state. The more complex the situation, the more moves we permit each set of decision makers, and the more uncertainty and strategic misrepresentation we admit, the more likely it is that the necessary deduction will contain an explicit mathematical component. Although the critics of economists do not like to acknowledge it, the adoption of ever more complicated mathematics by the latter was only partly motivated by "physics envy"; it was also prompted by their inability to answer complicated questions in natural language. As Donald McCloskey argues in a work that is quite critical of the excesses to which mathematics can

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lead, the consequence of the primitive machinery available for the conversation that represented premathematical economics was an inability to speak clearly.[7]

Mathematics could help advocates of the psychological/case-study archetype to speak more clearly about the impact of a given variable, to specify the interaction that produces contingent effects, and to forecast the impact of different strategies in situations to which there is no obvious historical or reliable experimental analog. Granted that there are many instances in which the key to policy is likely to rest on factors that cannot be easily deduced from an axiomatic model: the consequence of a leadership change, the domestic costs of initiating a conventional war, and so forth. Most axiomatic models, and certainly microeconomics, are theories of choice, not theories of preference change. But there are other situations in which the use of some quasi-axiomatic framework can be invaluable. Axiomatic analysis is required for determining how a Tit-for-Tat strategy should be adapted to conditions where the probability of misperception is high and to problems such as what mixture of sanctions and forgiveness of treaty violations is optimal, as well as how bargaining outcomes are affected by biased assessments of adversarial preferences. Granted, these questions will never be answered without information that comes from outside the narrow boundaries of the axiomatic apparatus, but that apparatus can provide powerful tools for analyzing the strategic implications of this information. Moreover, it can accomplish this under a variety of assumptions that are congenial with the perspective of the psychology archetype.

The faults of an unsophisticated model do not damn its underlying form, although they may well damn the researchers who persist in making simplifications that lead to poor predictions or bad policy prescriptions. Axiomatic modeling is a far more flexible tool than critics of the rational deterrence model imagine. True, the calculus thrives on optimization, but it is compatible with the addition of numerous constraints that collectively dull the effects of the optimization assumption to the point where they are unrecognizable and quite mild. As noted, recent axiomatic models deal with uncertainty and make no distinction between challenger and defender. There is nothing to prevent models of rational deterrence from incorporating the possibility of inducements as well as punishments to preserve peace, or from creating models that explore the role of domestic politics.

The frustration that the case-study advocates feel with the length of

[7] Donald N. McCloskey, *The Rhetoric of Economics* (Madison: University of Wisconsin Press, 1985), 3.

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time it takes to adopt these measures is understandable; but it is probably no greater than the frustration the rational deterrence people feel with the inability of the case-study school to mold what seems to them an endless series of low-level and sometimes contradictory empirical generalizations into a coherent theory from which implications can be inferred.

Other problems are even more analogous. George and Smoke point out that it is no easy matter for the rational deterrence theorist to decide whether an incorrect measurement or an omitted variable is the source of an incorrect prediction. Yet it is no easier for a psychologist or comparativist to decide whether his or her hypothesis is failing because of incorrect measurement of a variable included in the model, an unspecified contingency (i.e., omitted interaction term), or the effect of an omitted variable. Specification and indicator construction are a problem for case-study as well as for formal model aficionados.

Another parallel dilemma faced by both approaches illuminates the way they complement each other. The unitary-actor assumption of the rational deterrence theorists is constantly criticized by members of the psychology/case-study school who themselves do not hesitate to assume that a heuristic that is employed by an individual will also be employed by the decision-making apparatus of a state. Where does this confidence come from? Is it so unreasonable to assume that an institution will identify a heuristic that leads to perverse effects and take steps to eliminate the effects, if not the heuristic itself? If it is unreasonable, why is the psychologist taking the trouble to tell decision makers that they should be careful to compensate for bias caused by the heuristic?

Conclusion

To argue that advocates of the rational deterrence model and the psychology/case-study archetypes should put aside their differences would be gratuitous and probably counterproductive. The history of science suggests that such heterogeneity can be most helpful in the early stages of a field's development. Both biology and physics benefited from this heterogeneity. Indeed, it is difficult to think of many sciences that have had a purely axiomatic or a purely experimental genesis. Those inclined to narrow descriptions of what constitutes scientific theory and the way that it "always" develops would do well to consider the differences between the theory and the methods of Newton and Darwin—or, for that matter, the difference between Newton's *Optics* and *Mechanics*.

A commitment to either approach should not, however, blind us to the benefits of exploring their interrelationships. If the psychologists and comparativists succeed in identifying biases and in convincing decision

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makers to reduce them, they will have created a world that is more consistent with the underlying assumptions of rational deterrence models.[8] That is, if the United States and the Soviet Union both permitted their foreign policy decision making to be improved by psychologists, the attribution bias and other heuristics that now prevent rational deterrence models from accurately describing the way decision makers operate would be eliminated, and the rational deterrence model would perform better and be more useful than it is at present. In short, the more successful the psychologists are in accomplishing their mission of blunting the impact of biases, the weaker their critique of the proponents of formal modeling will become.

The irony of this situation continues because the very existence of the psychological approach provides a rationale for exploring the implications of rational deterrence models that is often missed. Case-study advocates and psychologists often argue that rational models are less relevant to the world of international relations than to economics because, in the former, there are no competitive forces generated by a market to pressure nations to adopt "rational" behavior. That is a useful insight. Yet the fact that these scholars are at times heeded by decision makers is evidence that a market is *not* a necessary condition for the adoption of more optimal decision rules regarding the use of deterrence. This suggests that there are cases where the presence or absence of a market may affect the rate at which decision rules converge on optimality more than the question whether such a convergence takes place. All that is required is the recognition that a dangerous bias exists—together with the capacity to eliminate its effect. The case-study school has played a critical role in developing this recognition.[9]

In this instance, at least, the relationship between the two archetypes is not only paradoxical, it is symbiotic. Rational deterrence theory will not describe the way the world works until certain heuristics and biases that can only be discovered through case studies and other inferential methods are abandoned. Psychologists and case-study researchers will find it difficult to trace through the implications of their discoveries for strategic behavior until they adopt some close relative of formal deductive methods. With luck, these schools may jointly provide a basis for developing properly specified "strong" models of deterrence.

[8] For internal reforms of institutions and decision-making processes designed to minimize biases, see Alexander L. George, "The Case for Multiple Advocacy in Making Foreign Policy," *American Political Science Review* 66 (September 1972), 751-85; Irving L. Janis, *Victims of Groupthink* (Boston: Houghton Mifflin, 1972); and Robert Jervis, *Perception and Misperception in International Politics* (Princeton: Princeton University Press, 1976).

[9] The author is developing this line of argument in a paper titled "Bias, Uncertainty, and Theories of Rational Deterrence."

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POWER, CAPABILITIES, AND PARADOXICAL CONFLICT OUTCOMES

By ZEEV MAOZ*

All the horrors of war stem from men's efforts to prevent it.
—*J. J. Rousseau*[**]

I. Introduction

POLITICAL leaders operate, plan, and implement policies whose underlying logic is "more is better." So they accumulate more arms, more sophisticated weapons, and larger armies. But sometimes, when the real test comes, it turns out that not only is more not better, but more might actually be worse. The paradox of conflict outcomes is more severe—and certainly much more mind-boggling—than the observation that states with greater capabilities lose wars against militarily inferior adversaries: it is that such states may lose wars *because* they possess superior capabilities.

The purpose of this study is threefold. First, I wish to show that excessive control over resources may sometimes cause the loss of control over outcomes; that it is not unreasonable to expect such a result in theory or in practice; and that knowing that the paradox of power and conflict outcomes is a distinct possibility is not a sufficient condition for resolving it. Second, I offer two possible explanations of this paradox and apply them to two empirical paradoxes of power in international politics. Third, I discuss the implications that these issues have for the study of international conflicts.

II. Paradoxes, Power, and Paradoxes of Power

The dictionary defines the term "paradox" as a contradictory or absurd statement that might nonetheless be true.[1] While I do not wish to com-

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** C. E. Vaughan, *The Political Writings of J. J. Rousseau* (Cambridge: Cambridge University Press, 1915), 365.

[1] For other definitions of the term, and applications of paradoxical logic to the study of po-

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pete with linguists, philosophers, and others who have employed this term, I will employ a more stringent definition. In this study, a paradox will refer to a causally induced contradiction between expectations and the consequences of behavior resulting from them. This contradiction entails a causal process in the sense that a perfectly reasonable set of expectations, goals, or desires produces seemingly reasonable behavior, which results in outcomes that are not only different from those one wished to achieve, but are their precise opposite.

The centrality of power in the study of politics is second only to the number of definitions assigned to this concept. Yet, three conceptualizations of power seem to be common to the various definitions and empirical measures of the concept: (1) power as control over resources; (2) power as control over actors; and (3) power as control over outcomes.[2] The ability of actors to secure the desired outcomes or to prevent the occurrence of undesired ones is obviously the most sensible and widely accepted definition. Unfortunately, it also happens to be the most difficult one in its empirical referents. It is risky to measure power in terms of the potential to control outcomes because of possible tautologies—that is, inferences of power based on actual observation of outcomes. Since there are no straightforward ways of determining, a priori, the potential to control outcomes in an interdependent setting, it is difficult to tell which actor had more control over outcomes in a given situation unless one knows the outcomes that obtained.

Perhaps this difficulty of transforming the most sensible conceptualizations of power into empirically observable indicators gave rise to the widespread intuition that the three approaches to power are logically and empirically associated. Specifically, it is generally believed that this relationship—if not perfectly linear—is of at least a monotonically increasing functional form: the more resources an actor controls, the more likely it is to control the behavior of other actors, and, consequently, the more likely it is to determine the nature of outcomes in its environment. This intuition is shared by both scholars and practitioners of international politics. Attempts by students of quantitative international politics to measure national power have focused almost exclusively on various combi-

litical phenomena, see Steven J. Brams, *Paradoxes in Politics: Introduction to the Nonobvious in Political Science* (New York: Free Press, 1976); Robert E. Maydole, "Superpower Paradoxes" (mimeo., Davidson College, 1987); W. V. Quine, *The Ways of Paradox* (New York: Random House, 1965); Arun Bose, *Political Paradoxes and Puzzles* (Oxford: Clarendon Press, 1977); Edward N. Luttwak, *Strategy* (Cambridge, MA: Belknap Press, 1987).

[2] For a thoughtful discussion of these meanings of the term, see Jeffrey Hart, "Three Approaches to the Measurement of Power in International Relations," *International Organization* 30 (April 1976), 299-305.

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nations of national capabilities.[3] From the practical side, the underlying logic of nuclear and conventional arms races, as well as the pursuit of *Realpolitik* by foreign policy elites provides a clear substantiation of the influence of this intuition.

We can merge the concepts of paradox and power in order to understand the paradox of power. First, it is instructive to examine what a paradox of power is not. For various reasons, it is not merely a situation in which an actor with considerable control over resources has little control over outcomes. First, there are some outcomes that no amount of resources can change: both the richest and the poorest person on earth are going to wind up dead. Second, the possession of certain resources makes one more susceptible to certain undesirable outcomes than does the lack thereof: if we assume that both a rich person and a poor one prefer living over dying, the former is more likely to die in a plane crash than the latter because the former can afford to fly and the latter cannot. Third, the ability to control outcomes may require things other than the control over resources: actors who possess relevant resources may, because they do not wish to spend them, fail to obtain favorable outcomes. On a more general level, a discrepancy between control over resources and control over outcomes is, in itself, not sufficient for establishing the existence of a paradox of power in the sense in which the term "paradox" is defined here: it is necessary to demonstrate that a discrepancy between control over resources or actors and control over outcomes is causally induced. Specifically, it must be shown that increased control over resources (or over actors) *produces* reduced control over outcomes; that a stronger actor lost control over outcomes *because* it had increased its control over resources.

To establish such causality, three conditions have to be satisfied. (1) *Temporal sequence:* changes in the degree of control over resources (or over actors) must precede changes in the degree of control over outcomes. (2) *Logical exclusiveness:* no factor other than the change in resources is responsible for the change in the actor's control over outcomes. All other factors that could possibly cause changes in the actor's ability to control outcomes have been left constant. (3) *Substantive meaningfulness:* the discrepancy between control over resources (or actors) and control over outcomes must have a meaningful substantive explanation. This entails an explication of the processes and the logic that gave rise to the discrepancy.

[3] See, for example, J. David Singer, Stuart Bremer, and John Stuckey, "Capability Distribution, Uncertainty, and Major Power War, 1820-1965," in Bruce M. Russett, ed., *Peace, War, and Numbers* (Beverly Hills, CA: Sage, 1972), 19-48; A.F.K. Organski and Jacek Kugler, *The War Ledger* (Chicago: University of Chicago Press, 1980); Bruce Bueno de Mesquita, *The War Trap* (New Haven: Yale University Press, 1981).

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Thus, a paradox of power is one wherein a change in an actor's control over resources or over the behavior of other actors causes an inverse change in the actor's control over outcomes. Two results are possible: (1) an increase in the actor's ability to control resources or actors causes a decrease in the actor's ability to control outcomes, and (2) the inverse of (1): decreased control over resources or actors causes increased control over outcomes.

III. Extra Power Can Hurt

Actors who are already doing well sometimes want to go an extra step just to make sure that they will get what they want. Yet this extra step sets into motion processes that eventually lead to their undoing, and that would not have taken place if the focal actors had refrained from taking that crucial extra step. Such cases represent extremes. They do not deal with actors who were initially weak and are in a process of increasing their capabilities. Such actors may obviously harm themselves as they are drawn into conflict by stronger actors who feel threatened.[4] The case in which an actor is already more powerful than others in the sense that it has the potential of controlling outcomes is interesting because, if a paradox of power can be shown here, similar paradoxes may reasonably be expected to arise in more typical cases.

It is useful to introduce the paradox of power through game-theoretic analogies in the context of committee voting so as to stress the fact that it is not due to some sort of cognitive imperfection in the attributes or the functioning of the actors. In fact, the theoretical demonstrations of the paradox assume that all actors are rational, capable of sophisticated and strategic behavior, and fully informed of each other's resources and preferences. In addition, the rules of the games in which these paradoxes arise are such that choice is simultaneous and communication among actors (and consequently the making of binding commitments) is prohibited.[5]

The two formal versions of paradoxes of power in the sense defined above are known as the *monotonicity paradox* and the *chairman paradox*. Since formal presentations of these paradoxes have been published else-

[4] See Jack S. Levy, "Declining Power and the Preventive Motivation for War," *World Politics* 40 (October 1987), 82-107, for a review of the literature on this issue.

[5] I am using voting-related examples for two reasons. First, voting resources and voting power (in terms of control over outcomes) are some of the most simple and straightforward concepts to measure. This is contrary to concepts such as military capabilities and national power where no universally accepted definitions or measures exist. Second, the theory of sophisticated voting developed in Robin Farquharson, *Theory of Voting* (New Haven: Yale University Press, 1969) provides the most convenient setting to show the paradox of power in a manner that satisfies the three criteria mentioned above.

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where, I will confine the present discussion to the moral of these stories in order to explore the processes they seem to suggest.[6] Briefly, the monotonicity paradox describes a situation wherein extra control over actors results in loss of control over outcomes. A candidate who wins an election with m committee members voting, loses the election if k additional members join the committee, where all k members strictly prefer that candidate to any of the other candidates. The chairman paradox describes a process where the acquisition of extra voting-related resources results in loss of control over outcomes. A committee member who is given increasing voting-related resources (e.g., no voting right, one vote, one regular vote plus a tie-breaking vote) achieves increasingly worse outcomes with the increase in his resources. Both cases fully satisfy the conditions of temporal sequence and logical exclusiveness because the only thing that is being changed in the electoral setup is the amount of resources or actors under the control of the focal player.

The substantive process that takes place in both cases is suggestive of the processes that might give rise to paradoxes of power in world politics. The acquisition of extra resources by the focal actor drastically alters the stakes of the game for other actors because these resources enable the first actor to do things it could not have done before. The stakes are altered, not only due to the amount of resources under the disposal of the superior actor, but primarily because of this actor's preference structure. Thus, the acquisition of extra resources by the superior actor sets in motion a process of strategic behavior by inferior actors that is designed to offset the superiority of the focal player. Since the rules of the game prohibit formal coalition formation, strategic behavior amounts (at least in the case of the chairman paradox), to a process wherein inferior actors tacitly gang up against the superior one. The net result is that the superior actor achieves significantly worse outcomes with more resources than he would have gotten with less.

Although systematic assessments of the conditions under which paradoxes of power arise are not available at present, the structure of the known cases suggests that a paradox of power will not necessarily occur each time a superior actor increases its resources. Nor will a paradox arise

[6] The monotonicity paradox was proved by Dan S. Felsenthal and Zeev Maoz in "A Comparative Analysis of Sincere and Sophisticated Voting Under the Approval and Plurality Procedures," *Behavioral Science* 33 (April 1988), 116-30. For the original version of the chairman paradox, see Farquharson (fn. 5). Additional versions of that paradox are discussed in Steven J. Brams, Dan S. Felsenthal, and Zeev Maoz, "New Chairman Paradoxes," in Andreas Diekman and Peter Mitter, eds., *Paradoxical Effects of Social Behavior: Essays in Honor of Anatol Rapoport* (Vienna: Physica Verlag, 1986), 243-56, and Zeev Maoz and Dan S. Felsenthal, "Self-Binding Commitments, the Inducement of Trust, Social Choice and the Theory of International Cooperation," *International Studies Quarterly* 31 (June 1987), 177-200.

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each time a power transition takes place (that is, when the second-most-capable actor switches ranks with the most capable one). Rather, such a paradox is due to a combination of resources and preferences. It will occur only in cases where the increased control over resources creates outcomes that make other actors feel sufficiently threatened to behave strategically—that is, to choose second-best policies in order to avoid the worst.

IV. Explanations of Power Paradoxes

As I have argued, it is neither unreasonable nor irrational to expect that increased control over resources or actors will cause a decrease in the ability of actors to control outcomes. Still, it remains to be explained why sensible people who are capable of making sophisticated decisions would—consciously or unconsciously—harm themselves by increasing their capabilities regardless of the consequences of such policies. The argument that power paradoxes are compatible with a rational choice framework does not imply, however, that they arise only if everybody is rational. The following section provides two different interpretations of the factors that might lead actors into self-made traps.

A. The Rational Explanation

In the paradoxes of power discussed above, as well as in other paradoxes of rationality, proponents of the rational model argue that actors simply do not have much of a choice. The rules of the game are defined exogenously (by the interdependent nature of the system, by the structure of actors' preferences, or by external actors that are not an integral part of the paradoxical game). Since actors act under a predetermined set of rules, they cannot resolve the paradox if they do not control the rules of play.

The examples of sophisticated voting are presented as if focal actors had free choices among alternatives with varying degrees of control over resources or over other actors. The outcomes are seen as paradoxical or nonparadoxical, depending on the alternative of resources that a focal actor selects. If that were the case in reality, paradoxes would be easy to resolve. A rational actor who realizes that it is playing a game wherein extra control over resources can hurt should be willing to give up any resources or other prerogatives that would harm it. In many cases, however, actors do not have a free choice of resources. Even if such a choice were available, it might be rationally inadmissible because the paradoxical power game is only part of a larger strategic context. State *A* might elect to in-

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crease its resources because it feels threatened by state *B* although it knows that this increase of resources might get it into trouble with state *C*. Likewise, leaders may engage in excessive defense spending to gain domestic support, even though they know that this will destabilize their relations with a key opponent—a process they would like to avoid.

Treated in isolation, then, causally induced discrepancies between control over resources and control over outcomes are paradoxes. In a broader strategic context, however, these are understandable—if difficult—political tradeoffs. Yet, a paradoxical aspect still remains from the point of view of rational explanations: the overall result of the solution of the tradeoff in favor of more resources, at the expense of more favorable international outcomes, might still be Pareto-inferior. All actors involved might have been strictly better off under a different set of choices.[7]

B. The Cognitive Explanation

The cognitive explanation of power paradoxes is based on a familiar misperception involving relations between premises and conclusions. It runs as follows: if everybody agrees on the factual aspects of a given situation, then everybody will agree on the conclusion. The misperception is that agreement on facts does not imply agreement on the conclusions derived from them. Even if everybody agrees on the facts and draws the same logical conclusions, these conclusions are not equivalent to actual behavior. Logical conclusions form the basis for calculations prior to behavior, not the equivalent of behavior. Robert Jervis discusses a related problem of misperception which consists of a tendency of decision makers to assume that observable data that are consistent with one interpretation must invariably be inconsistent with another interpretation.[8]

The problem is not, however, that actors disagree on the interpretation of the distribution of resources; it is that they draw different conclusions from this distribution in terms of what must be done to control outcomes. There is some fundamental bias in a simplistic subscription of actors to a *Realpolitik* paradigm that associates control over resources with control over outcomes. This bias is reflected in prevailing deterrence policies and practices. The notion that one can reduce motivations of states to a simpleminded pursuit of resources leads to an unrealistic disregard of the fact that resources are merely means that can be used to accomplish desirable

[7] Maoz and Felsenthal (fn. 6) and Felsenthal and Maoz (fn. 6) show that both the monotonicity paradox and the chairman paradox can be combinations of personal power paradoxes and collective choice paradoxes in which the outcomes induced by selection of greater resources are socially inferior to those induced by the selection of lesser resources.

[8] Jervis, *Perception and Misperception in International Politics* (Princeton: Princeton University Press, 1976), 144-81.

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ends or to prevent undesirable ones from occurring; they are not ends in and of themselves.[9] Actors have preferences for certain outcomes; hence, their choice of resources should really be seen in the context of which outcomes can be secured by certain resources. Moreover, trouble may arise because the distribution of capabilities in a system is only a starting point for strategy planning, not the end result. Weak actors do not willingly submit to strong ones simply because they know they are weak; strong actors do not simply swallow up weak ones simply because they are strong. Simplistic extrapolation from capabilities to outcomes is not only unrealistic; it might be erroneous and very costly if relied upon. The question is: What are the options of weak actors and can strategy compensate for inferiority in capabilities?

States with superior capabilities that ignore the options and strategies available to their inferior opponents are likely to get into trouble—not because their perception of the balance of capabilities is off the mark, but because they may arrive at the wrong conclusions from the shared and agreed knowledge of the distribution of capabilities. Even if all players know the distribution of resources and prerogatives and conclude that the actor with the most resources has the greatest potential for controlling outcomes, they will not necessarily give up and go home. If they do not like the outcomes induced by the actor with the most resources, they will try to figure out a way of changing them. They accomplish this via strategy, not via submission to the conclusion. An actor working from a set of correct premises and logical conclusions might get itself into a trap of its own making if it assumes that the game ends there. The cognitive problem is that of failing to attribute to opponents the ability to adjust their strategies to an environment in which they become increasingly inferior.

How does one show the occurrence of these paradoxes in practice? The tightly structured setting of committee voting has little in common with the chaotic and anarchic international arena. Many of the assumptions that seem reasonable in the context of committee decision making are quite far-fetched—if not totally irrelevant—in an international context. It might be argued that, given the substantive explanations of the discrepancies between control over resources and control over outcomes, the paradox of power is a veridical one. The proof contains a hidden element;

[9] On the tendency to interpret intentions from capabilities in the context of deterrence, see Robert Jervis, "Perceiving and Coping With Threat," in Robert Jervis, Richard Ned Lebow, and Janice Gross Stein, *Psychology and Deterrence* (Baltimore: The Johns Hopkins University Press, 1985), 13-33. Also see Janice Gross Stein, "Calculation, Miscalculation, and Conventional Deterrence," *ibid.*, 34-88, and "Deterrence and Reassurance," paper presented to the committee on the contribution of the behavioral sciences to the prevention of nuclear war, National Research Council, 1987.

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once we ponder the proof, the surprise fades.[10] In this case, explanations that focus on tacit collusion of inferior actors against superior ones suggest that we have a balance-of-power argument in disguise. In order to dispel such accusations, we must discuss possible paradoxes of power in international politics, and see how the abstract explanations can help us understand the dynamics of national self-entrapment.

V. Paradoxes of Power in International Conflicts

Jeoffrey Blainey has pointed out that both sides in a war usually believe that they will emerge victorious.[11] It is obviously impossible that both sides will be objectively correct. However one chooses to measure control over resources, in international conflicts this conceptualization of power entails some combination of military, economic, demographic, and political capabilities. If the widely shared intuition of the monotonically increasing association between control over resources and control over outcomes has some validity, then actors with superior capabilities should emerge victorious when these capabilities are put to the test in international conflict. That this is not always the case should not surprise anyone familiar with the scientific study of international conflicts. By now there is a considerable body of evidence—encompassing many studies, measures of capabilities, levels of conflict, and datasets—that suggests that the relationship between control over resources and control over conflict outcomes is either weak or nonexistent.[12] Those who are not satisfied with rigorous evidence emerging from quantitative analyses need only look at the performance of the nuclear powers in the post-World War II era. The United States, with a near monopoly over nuclear weapons, was barely able to get a draw in the Korean War and was badly beaten in Vietnam. The French and British forces got a bad beating in Suez. France got burned in Indochina and Algeria. The Soviet Union is still trying to extricate itself from Afghanistan. China's adventure in Vietnam in 1979 was far from a success. Even if these examples do not constitute a major-

[10] For a discussion of the types of paradoxes, see Maydole (fn. 1), 5, and Quine (fn. 1).

[11] Jeoffrey Blainey, *The Causes of War* (New York: Free Press, 1973).

[12] Robert Mandel, "The Effectiveness of Gunboat Diplomacy," *International Studies Quarterly* 30 (March 1986), 59-76; Zeev Maoz, *Paths to Conflict: International Dispute Initiation, 1816-1976* (Boulder, CO: Westview Press, 1982); Zeev Maoz, "The Expected Utility of International Conflict: Some Logical Traps and Empirical Surprises in 'The War Trap' " (mimeo., University of Haifa, 1984); Zeev Maoz, "Resolve, Capabilities, and the Outcomes of Interstate Disputes, 1816-1976," *Journal of Conflict Resolution* 27 (June 1983), 195-225; Frank W. Wayman, J. David Singer, and Gary Goertz, "Capabilities, Military Allocations, and Success in Militarized Disputes," *International Studies Quarterly* 27 (December 1983), 497-515.

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ity of cases in terms of frequency counts, they beg for an explanation.[13]

Because it is not accompanied by a clear causal structure, this evidence does not constitute an empirical validation of paradoxes of power. It does suggest that a more thorough examination of the widely shared intuition is in order. Any attempt, however, to establish the causal elements entailed in the paradox of power in a historical context runs into serious problems. The abstract examples, cited to satisfy the logical exclusiveness condition, show which outcomes obtain under a given level of control over resources. Reality does not afford such flexibility. We know what actually happened, but it is impossible to tell with any degree of confidence what would have happened if the superior actor had had less control over resources or actors. So we need to take an indirect route to empirical demonstration.

We know that militarily superior states may perform poorly in conflicts against weak opponents. Since capabilities are measured prior to the outbreak of actual hostilities (and do not change drastically in low-level conflicts or during brief wars) the existence of a clear temporal sequence in which control over resources precedes the loss of control over outcomes can be easily established. The problem—to demonstrate that superior capabilities *caused* the focal actor to lose control over the outcome of the conflict—makes the third criterion of substantive meaningfulness particularly important. In order to suggest that the paradox of power provides a potential explanation of a given historical episode, we must show that events in that case followed a certain sequence. This sequence is similar or identical to what one would expect according to the substantive interpretation of the process that gave rise to the power paradox in the abstract examples. If that is the case, we cannot rule out the abstract process outlined by the power paradox as a plausible explanation.[14]

Thus, several points must be established in an empirical analysis of the power paradox: (1) An actor increases its control over resources relative to other actors. (2) The actor's resources are superior to those of any of the other actors, but they are not necessarily superior to those of all other relevant actors combined. (3) Prior to the focal actor's acquisition of extra control over actors or resources, other actors were not in a coalition—for-

[13] See Andrew J. Mack, "Why Big Nations Lose Small Wars: The Politics of Asymmetric Conflict," *World Politics* 27 (January 1975), 175-200.

[14] This kind of historical analysis follows what Alexander L. George and Timothy J. McKeown, "Case Studies and Theories of Organizational Decision Making," in Robert Coulam and Richard Smith, eds., *Advances in Information Processing in Organizations*, Vol. 2 (Greenwich, CT: JAI Press, 1985), 29-34, call the "congruence procedure." The idea is that a theoretical framework postulates a set of expected outcomes in one or more cases, and the fit between the expected and the actual outcomes serves as a basis for the assessment of the framework.

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mal or informal—directed at it. (4) There is some evidence of formal or tacit collusion of the inferior actors against the superior actor as a result of (1). (5) Because of this collusion, the focal actor loses control over outcomes. (6) Once this happens, the formal or informal collusion among the inferior actors ceases.

Granted, the correspondence between the sequence of events in the abstract cases and the sequence of events in the historical cases does not make the paradoxical explanation an exclusive one. Nor does it make for the best possible explanation of the historical facts. But since my objective is to suggest some new ways of thinking of the relationships between resources and outcomes in world politics, I shall be satisfied with showing that the power paradox provides a plausible interpretation of seemingly puzzling events in history. My examples are the two Balkan Wars of 1912-1913 and Israel's invasion of Lebanon in 1982-1985.

A. On the Art of Dividing Spoils: The Balkan Wars, 1912-1913[15]

The story of the Balkan Wars should be of great interest to students of international politics, for several reasons. First, both wars are cases in which weaker actors prevailed over stronger ones. In the first war, the coalition of Bulgaria, Serbia, and Greece took on the decadent but still strong army of Turkey. In the second war, Bulgaria, the big winner of the first war, became the big loser when it fought against its former allies, Serbia and Greece, against its former enemy, Turkey, and against a former nonbelligerent, Romania. Second, the Balkan Wars constitute an interesting case of alliance politics, and how these politics relate to war.[16] Third, the entire period before, during, and following the wars was characterized by intense multilateral negotiations, both among the various Balkan states and between the Balkan states and the Great Powers. It is surprising, therefore, that few analytical efforts have been made to analyze these wars, especially in terms of how they relate to the outbreak of

[15] This analysis is based on the following sources: Richard J. Crampton, *Bulgaria, 1878-1918: A History* (Boulder, CO: East European Monographs, distributed by Columbia University Press, 1983); Douglas Dakin, *The Unification of Greece, 1770-1923* (London: Ernest Benn, 1972); Vladimir Dedijer et al., *History of Yugoslavia* (New York: McGraw Hill, 1974); Diplomatist (anonymous), *Nationalism and War in the Near East* (Oxford: Clarendon Press, 1915); Ernest C. Helmreich, *The Diplomacy of the Balkan Wars, 1912-1913* (New York: Russell & Russell, 1969); William Miller, *The Ottoman Empire and Its Successors, 1801-1927* (London: Frank Cass, 1966); Michael Boro Petrovich, *A History of Modern Serbia, 1804-1918*, Vol. II (New York: Harcourt Brace Jovanovich, 1976); and Leon Trotsky, *The Balkan Wars, 1912-1913* (New York: Monad Press, 1980).

[16] The Balkan Wars also shed considerable light on what Bruce Bueno de Mesquita (fn. 3) considers a counterintuitive aspect of international politics: war among allies.

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World War I.[17] While I do not wish to correct this deficiency in the present discussion, I do intend to show how these wars can be understood in terms of a paradox of power, and how the rational and cognitive explanation of this paradox can shed light on the causes and consequences of these two wars.

The First Balkan War broke out after a period of intense bargaining among the three Balkan states—Bulgaria, Serbia, and Greece—and Turkey. This process was closely monitored by the Great Powers—Austria-Hungary, Russia, Germany—and, to a lesser extent, Great Britain and France. Formally, the issue under negotiation concerned the rights of the Christian population in the area controlled by Turkey in Europe. The real issue, however, was that of the Turkish territories in Europe, populated by Slavic peoples of very diverse origins, and with affiliation to various Balkan governments. All three states felt that the time was ripe to acquire these territories from Turkey, which had been weakened by corruption and domestic turmoil since the Young Turk revolution in 1908. Throughout the spring of 1912, intense negotiations took place between Bulgaria and Serbia on the one hand, and between Bulgaria and Greece on the other, to establish a joint front against the still overwhelmingly large Turkish army. On March 13, 1912, an alliance treaty was signed between Bulgaria and Serbia, and on May 17, 1912, a similar treaty was signed between Bulgaria and Greece.[18] Bulgaria was the pivotal element in the Balkan alliance; Serbia and Greece had no alliance with each other.

The Great Powers immediately realized the offensive nature of these alliances and began a series of mediation efforts to reduce the risks of war in the Balkans. These efforts were half-hearted, however, because the Great Powers were themselves split in their views of an appropriate solution to the problem. Russia was interested in a reduction of the Turkish rule in Europe and in good relations with the Balkan states. Austria-Hungary was concerned about the risk of a "greater Serbia" and its implications for an old and ethnically divided empire. Nonetheless, the Great Powers issued a joint proposal designed to bring about a peaceful

[17] Works on international crises covering this period have generally ignored these two wars. For example, Glenn Snyder and Paul Diesing, *Conflict Among Nations* (Princeton: Princeton University Press, 1977) mention these wars only in passing, in the context of Great Power diplomacy (p. 433). Richard Ned Lebow, *Between Peace and War* (Baltimore: The Johns Hopkins University Press, 1981) ignores these wars completely. For an exception, see the discussion of the series of crises leading to World War I that deals explicitly with the Balkan Wars in Alan Ned Sabrosky, "From Bosnia to Sarajevo: A Comparative Discussion of International Crises," *Journal of Conflict Resolution* 19 (March 1975), 3-24.

[18] Although Montenegro was another participant in the alliance systems leading to the First Balkan War, it will not be discussed for two reasons. First, it was the weakest link in the chain, its capabilities having little effect on the total balance of forces. Second, it was heavily under Serbian influence, rendering its independence of choice questionable.

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settlement of the dispute. This proposal, submitted to the parties on October 8, 1912, contained an explicit Great Power commitment to the preservation of the territorial status quo in the Balkans.[19] It suggested that the Balkan allies might face strong opposition by the Great Powers—if not to a war, then to any shrinkage in Turkey's European possessions. Even without this opposition, the combined forces of the Balkan states were not even close, numerically speaking, to the Turkish army. (See Table 1.)

Turkey was not only numerically superior to each and every member of the Balkan coalition; it was superior to all three Balkan states combined. Notwithstanding the Great Powers' commitment to the status quo or Turkish numerical superiority, the Balkan states declared war on Turkey (with the Montenegro declaration on October 13, and the Greek, Serbian, and Bulgarian declarations five days later). The war had two phases. The first, which lasted until late November 1912, saw a rapid advance by the three Balkan armies and a hasty retreat by the Turkish army after a series of defeats. With the siege of Adrianople by the Bulgarian army, and with the way to Constantinople virtually open, it was clear that the war was over for Turkey. Indeed, the Turkish government appealed to the Great Powers for an armistice, which was granted. Attempts to resolve the territorial disputes through negotiation in London failed, however, and battles resumed on January 28. At that time, the military objectives of the war were accomplished from the Serbian and Greek perspective, but Bulgaria still had unfinished business with Turkey in Adrianople. The occupation of Adrianople was completed on March 30, 1913, and the London conference resumed in early April.

Turkey lost the First Balkan War because its army was technologically and organizationally weak; because political chaos pervaded the army's command; and because the quality of the troops and weapons was poor. Diplomatist quotes another anonymous authority on military affairs of the time on the explanation of the Bulgarian success in the war: "Superior numbers are an undoubted advantage; but skill, better organization and training, and above all, a firmer determination in all ranks to conquer at any cost, are the chief factors of success."[20] But the territorial gains of the First Balkan War were unequal, in terms of what the allies had set out to do and in terms of what they eventually accomplished. Bulgaria was clearly the biggest winner. In fact, Bulgaria emerged from the war as arguably the most powerful state in the Balkans except for Romania. If territory and population are elements of national resources, all the allies

[19] The text of this proposal may be found in Helmreich (fn. 15), 130.
[20] Diplomatist (fn. 15), 192.

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TABLE 1
THE BALANCE OF FORCES IN THE BALKANS, 1908-1913

	Turkey			*Bulgaria*			*Greece*			*Serbia*			*Romania*		
	Mil. Pers.[a]	*Mil. Exp.*[b]	*Total Cap.*[c]	*Mil. Pers.*[a]	*Mil. Exp.*[b]	*Total Cap.*[c]	*Mil. Pers.*[a]	*Mil. Exp.*[b]	*Total Cap.*[c]	*Mil. Pers.*[a]	*Mil. Exp.*[b]	*Total Cap.*[c]	*Mil. Pers.*[a]	*Mil. Exp.*[b]	*Total Cap.*[c]
1908	319	12908	0.55	59	1203	0.08	25	1148	0.05	29	8620	0.19	94	2148	0.13
1909	312	13728	0.64	60	1456	0.10	25	1106	0.05	36	1070	0.06	104	220	0.15
1910	324	9436	0.60	61	1586	0.10	30	1373	0.07	35	1031	0.07	89	2425	0.16
1911	336	10637	0.61	61	1586	0.10	35	1182	0.07	30	1071	0.06	97	2746	0.17
1912	341	9203	0.59	58	1620	0.10	29	1202	0.06	31	1184	0.06	100	2951	0.18
1913	341	9203	0.57	55	1620	0.10	27	1920	0.08	32	1194	0.06	109	3236	0.19

Source: Correlates of War Project, "National Capabilities 1816-1980" (Ann Arbor: University of Michigan, 1986).

[a] Number of soldiers in active army (in thousands). These numbers refer only to the regular armies, not to the size of the mobilized forces of the combatants during the wars.

[b] Military expenditures in thousands of pounds (sterling).

[c] Average proportion of military capabilities (personnel and expenditures), expressed as percentage of the total of the states in the table. This method of computation has been used by various authors—for example, Singer, Bremer, and Stuckey (fn. 3), Bueno de Mesquita (fn. 3), and Maoz (fn. 12, 1982).

gained in the war. But Bulgaria gained the most, increasing its resources by a larger relative margin than any of its allies.

This, indeed, was Bulgaria's problem. During the negotiations in London, the former allies began to debate the division of the spoils. Serbia and Greece felt that the territorial gains awarded to Bulgaria by the Great Powers were excessive, especially because the latter would not let them have additional territory. Serbia believed it should be compensated for not having gained territorial access to the Adriatic Sea. Greece felt deprived in that it was forced to give up territory acquired in Thrace and Macedonia. As if this were not enough, Romania joined the list by requiring revisions in its border with Bulgaria. The latter rejected all claims by its former allies; while agreeing to some modifications of its border with Romania, it refused to make sufficient concessions to pacify the Romanian government. The Great Powers showed some sympathy to the Bulgarian position; in particular, both Austria-Hungary and Germany were willing to support Bulgaria in order to frustrate Serbia. They urged the Bulgarians, however, to make concessions to the Romanians—a policy that Bulgaria did not want to consider.

Even before the postwar negotiations ended in the Treaty of London on May 30, 1913, Serbia and Greece started separate talks on an anti-Bulgarian alliance. They also sought Romanian support. On June 1, the Serbo-Greek alliance was signed. A tacit understanding was established with Romania that it would join in the event of a war. With Romania included, the new coalition was clearly superior to Bulgaria; without Romania, it would have been militarily inferior. While the Greek and Serbian armies started a process of demobilization (having concluded their part in the First Balkan War in November 1912), the Bulgarian army was still fully mobilized. In fact, in June 1913 it was slightly larger than it had been before the start of the first round.[21] Facing a hostile alliance and sensing a momentary advantage (or perhaps a necessity to pre-empt), the

[21] The Bulgarian army numbered some 600,000 troops (as opposed to 592,000 during the first war); the Greek army was reduced to 200,000 troops (from 215,000 during the war), and the Serbian army to 350,000 (from a total of over 400,000 during the war). These figures from Crampton (fn. 15), 417-20, generally agree with those in Petrovich (fn. 15), 603, who estimates the strength of the Romanian army at 437,000 troops and of the Turkish army at 255,000. Yet these numbers differ markedly from the estimates of the combined strength of the combatants in the First Balkan War given by other authors. Trotsky (fn. 15), 139-40, relying on a noted German military commentator, gives the size of the various armies at the start of the First Balkan War as follows: Bulgaria 200,000 (160,000 combatants); Serbia 120,000 (95,000 combatants); Greece 55,000 (45,000 combatants); Montenegro 35,000; Turkey 450,000 (360,000 combatants). These may be gross underestimates. On p. 272, Trotsky puts the total Bulgarian casualties in the First Balkan War at 102,000, suggesting that the size of the Bulgarian army was at least twice the initial estimate. Diplomatist (fn. 15), 176, cites an interview in *Near East* in the winter of 1911-1912 with a leading Young Turk, who estimated the mobilization capacity of the allies at 450,000 and the Turkish mobilization capacity at 800,000.

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Bulgarian army launched simultaneous attacks on the Greek and Serbian armies on June 30, 1913. The result was a general collusion against Bulgaria: Romania jumped into the battle, and so did Turkey—which, only a month earlier, had signed the Treaty of London, which included an agreement to cede Adrianople to Bulgaria. Within two weeks, Bulgarian forces were defeated on all fronts; the most embarrassing defeat was Turkey's recapture of Adrianople. The treaties of Bucharest (August 10, 1913) and Constantinople (September 29, 1913) granted the victorious parties almost the whole area they had been able to occupy. By refusing to give in to Serbian and Greek demands, Bulgaria lost not only these territories, but also most of what it had gained in the First Balkan War (in addition to having to cede a sizable piece of real estate to Romania).[22]

Let us examine whether this case corresponds to our power paradox. First, it is evident that the temporal sequence matches that which is described in the abstract example. Bulgaria gained considerable resources following the First Balkan War, both relatively and absolutely. Moreover, the Treaty of London legitimized Bulgaria's gains, and Austria-Hungary gave it strong support vis-à-vis Serbia and Greece. On May 29, 1913, the Austrian and German ambassadors to Romania issued a strong warning designed to deter that country from entering a war between Bulgaria and its former allies—which suggests that Bulgaria had secured some additional control over actors. The increase in control over resources and actors was followed by both a formal and a tacit collusion among actors that—except for the indirect alliance between the Greeks and the Serbs through Bulgaria—had not been allied with one another in the past.[23] This collusion resulted in the diminution of the previously superior actor. Bulgaria would have been better off (even without taking into account some 25,000 Bulgarian fatalities in the Second Balkan War) had it ceded the disputed territories to Romania, Serbia, and Greece.

Second, it is evident that this collusion was a direct result of the First Balkan War, and that it was aimed at the biggest winner. This gang-up operation did not last long, however. Two years after the Treaty of Constantinople, Bulgaria and Turkey found themselves fighting side by side

[22] Following the two Balkan Wars, the Bulgarian balance sheet did show some surplus. However, it was not only much smaller than it had been at the end of the First Balkan War, but it registered the smallest relative gain of all the allies in the first war. Helmreich (fn. 15), 453, shows changes in territory and population as a result of the two Balkan Wars. According to his figures, Bulgaria's territory was expanded by nearly 29%, and its population by 3%. This compares to a territorial expansion of 67% and a demographic expansion of 63% of Greece; a territorial expansion of 61% and a demographic expansion of 100% of Montenegro; and a territorial expansion of 81% and a demographic expansion of 55% of Serbia.

[23] In fact, Helmreich, *ibid.*, 350-51, reports that secret discussions between the two former enemies, Serbia and Turkey, took place throughout June 1913, in the best spirit of *Realpolitik*.

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against Serbia, with Romania and Greece cheering them on. Thus, the substantive interpretation of the power paradox seems to hold: increased control over resources results in a collusion among inferior actors, which offsets the capability of the stronger actor. Third, the increase in control over actors (Bulgaria's securing of Austro-Hungarian and German support) was not reliable; all it did was to generate misperception of the extent to which the Great Powers' statements would be effective in keeping Romania out of the war.

But why did the Bulgarians fail to respond moderately to their former allies' claims? Why did they plunge, head first, into disaster? The rational model suggests that the Bulgarian government was locked into a no-win situation: it had dragged the nation into a costly war, and the only justification for the losses of the war were the territorial gains it was able to secure in London. To give these up was seen as tantamount to treason. By late June, the Bulgarian government was faced with a proposal for Russian arbitration which it considered equivalent to a decision in favor of its former allies. It perceived that it had the choice of giving up Thrace and Macedonia or a war in which, given its superior capabilities, it had at least a fighting chance. The army could not justify continued mobilization following the Treaty of London. The prospect of prolonged arbitration, which was doomed to fail, suggested that war was inevitable and that remobilization would probably be required. War now was seen better than war later.[24] Although the Bulgarians were surprised by the Turkish attack, they were not surprised by the Romanian intervention. They did hope to launch a decisive attack on Serbia and Greece that would take these two opponents out of the game. They also hoped that Romania would remain on the sidelines until the dust of battle cleared and would refrain from action if Bulgaria's offensive was successful. In fact, Romania did not enter the war until July 15, but by then Bulgaria had suffered major defeats in the south and west.

The cognitive explanation suggests that the Bulgarian leaders, especially King Ferdinand and commander-in-chief of the army Savov, were carried away by military considerations. The Bulgarian cabinet was not told of the planned attack. The conflict between the cabinet and the king resulted in Savov's dismissal by the king on July 3. In addition, there was evidence of wishful thinking on two issues: the first was on Romanian attitudes and actions; the second was overconfidence in the ability of the Bulgarian army to defeat the Serbs and Greeks before the Romanians could mobilize.[25] Finally, as Helmreich argues, the king and Savov

[24] *Ibid.*, 359-62.

[25] *Ibid.*, 363-67; Crampton (fn. 15), 424.

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viewed the attack as part of a strategy designed to strengthen Bulgaria's position in an ensuing arbitration. They hoped for a Russian intervention designed to stop the war and to enforce a far more favorable settlement, taking into account Bulgarian accomplishments during the brief hostilities. This was indeed a case of wishful thinking; the Russian foreign minister, Sazonov, had informed Bulgaria that as a result of its position regarding arbitration, Russia had no intention of getting involved in the conflict.[26]

B. Israel's Lebanon Adventure: Anatomy of a Minor Disaster[27]

Israel had planned the invasion of Lebanon for a long time. The secret agreement between Israel and the Phalange leader Bashir Jemayel (signed in February 1982) reveals Israel's war aims quite clearly: (1) expulsion of the P.L.O. from Lebanon; (2) expulsion of the Syrian forces from Lebanon; (3) helping the Christian militia to occupy Beirut for the purpose of establishing a central, Christian-controlled government that would (4) stabilize the country and sign a peace treaty with Israel.[28]

Nobody doubted that Israel had the military capability to carry out a strategy of all-out confrontation with the Syrians and the P.L.O. The Syrians knew it, the Palestinians knew it, and the Christians knew it; otherwise, they would not have signed the agreement.[29] Rumors of a large-scale Israeli action in Lebanon circulated throughout the Middle East in the spring of 1982; no one really doubted Israel's intentions. Defense

[26] Helmreich (fn. 15), 366-67. "Now, after your declaration [requesting Russia to state its position within ten days] I communicate ours to you! Do not expect anything from us, and forget the existence of any of our engagements from 1902 until today." Quoted, *ibid.*, 361.

[27] The discussion of Israel's misfortune in Lebanon is based on Itamar Rabinovich, *The War for Lebanon, 1970–1983*, rev. ed. (Ithaca, NY: Cornell University Press, 1985); Emile F. Sahliye, *The PLO after the Lebanon War* (Boulder, CO: Westview Press, 1986); Zeev Schiff and Ehud Yaari, *Israel's Lebanon War* (New York: Simon and Schuster, 1984); and Avner Yaniv, *Dilemmas of Security: Politics, Strategy, and the Israeli Experience in Lebanon* (New York: Oxford University Press, 1987). These books offer detailed interpretations of the war seen from both the Israeli perspective and the perspective of the other participants in the war. Maoz and Felsenthal (fn. 6) analyze the strategic process leading to Israel's decision to withdraw from Lebanon in 1985.

[28] Yaniv (fn. 27), 100-103.

[29] Avner Yaniv, *Deterrence Without the Bomb: The Politics of Israeli Strategy* (Lexington, MA: D.C. Health, 1986), 196-200, provides a telling estimate of the balance of power in the region. Israel's defense outlays increased from $1.5 billion in 1980 to $8 billion in 1982. The combined military outlays of Egypt, Syria, and Iraq dropped from $6.4 billion in 1980 to $5.7 billion in 1982. The relative capabilities of Israel and Syria are measured in Zeev Maoz, "The Evolution of Syrian Power," in Moshe Maoz and Avner Yaniv, eds., *Syria Under Assad: Domestic Constraints and Military Risks* (London: Croom Helm, 1985), 78-80, using a modified version of the Correlates of War composite capabilities index (with GNP substituting for the traditional energy consumption and iron-steel production indicators of economic capabilities). According to these figures, Israel's capability in 1982 accounted for about 15% of the total capabilities of a reference group including Syria, Turkey, Egypt, Jordan, and Iraq. Syria, on the other hand, accounted for 7% of the combined capabilities of this reference group.

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Minister Sharon assumed that neither the Soviet Union nor any of the Arab states would actively support the Syrians and the Palestinians, and this assessment turned out to be correct.[30] The ability to secure secret U.S. support for a limited operation suggests that Israel had nearly everything working for it. Initially, everything went well for the Israelis. The Israeli army, despite heavy fighting with the P.L.O. and the Syrian forces, was able to advance rapidly into Lebanon, pushing the Palestinian guerrillas into West Beirut, destroying Syrian anti-aircraft missile batteries, and pushing Syrian ground forces into northeastern Lebanon. Within a week, the Israeli forces were in control of the southern part of Lebanon and of East Beirut and putting the remaining Palestinian forces under siege. Following a period of prolonged negotiations under the auspices of Philip Habib, the U.S. special envoy, the Israelis were able to secure the evacuation of the Palestinian forces from West Beirut. The Lebanese parliament elected Bashir Jemayel to the presidency, and the Israeli government was elated. It had every reason to believe that, as Prime Minister Menachem Begin put it, "the country shall be peaceful for forty years."

Just when it seemed that Israel was in a position to dictate the political future of Lebanon through its Christian proxies, the situation began to crumble. It all started with the assassination of Bashir Jemayel, and the ensuing massacres at Sabra and Shatilla. The Christians' grip over Lebanese politics began to weaken, and Syria's involvement in the internal struggle became more evident. The Israeli army got entangled in local struggles among various ethnic militias (initially in the Shuff mountains, between Druzes and Christians, and later in southern Lebanon, between Shi'ites and Christians). A strong Christian-controlled central government never emerged. Palestinian guerrillas began to infiltrate southern Lebanon in ever-growing numbers, launching an effective guerrilla campaign, in close collaboration with the Shi'ite militia, against the Israeli troops in the area. What was supposed to be a "quick and efficient" military operation gradually turned into a military and political disaster lasting nearly three years. When Israel pulled out of southern Lebanon in June 1985, it was no better off than when it had started the invasion, especially when the economic costs of the war and the concomitant social turmoil in Israel are considered. None of the far-reaching war aims had been accomplished. How can this collapse of Israel's grand strategy be explained?

A plausible interpretation is that, when it seemed that the Israelis were in a position to instate a strong Christian-controlled government in Leb-

[30] Yaniv (fn. 27), 102-7.

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anon, and that such a government would have to rely on a continued alliance with Israel, the other ethnic groups in the country got together. Previously, they had been engaged in a fight of all against all, but they found the common threat of Christian-Israeli domination far more daunting than either that of Syrian involvement or of an unstable state. A tacit collusion therefore aimed at getting the Israelis out of Lebanon, thereby weakening the Christian domination of the government. The Syrians were happy to participate in the struggle against the Israeli presence as long as they could find someone else to do the job for them. The P.L.O. was happy to supply both the logistics and the manpower in order to get back into the game of Lebanese politics. The policy of the new president, Amin Jemayel, shifted against Israel and toward increased cooperation and appeasement of the Syrians. Although the hit-and-run guerrilla operations did not do any significant strategic damage to the Israeli army, they were effective in causing numerous casualties and considerable demoralization of the army and of Israeli society in general. When the Israelis withdrew from Beirut and the Shuff area, leaving the latter under Druze domination, the Druzes calmed down. But at the same time, participation of the Shi'ite population in the guerrilla warfare in southern Lebanon escalated sharply. Not only did the Shi'ites actively participate in the struggle; they also provided shelter and logistical support to Palestinian guerrillas who infiltrated southern Lebanon from the Syrian-controlled Bekaa Valley and from northern Lebanon.

Although there is no clear proof that Israel's capabilities had a direct negative effect on its ability to accomplish a favorable outcome, the tacit collusion between the factions that had been bitter enemies prior to the Israeli invasion suggests that this effect cannot be ruled out. The fact that this collusion began just when Israel removed all the barriers for a Christian-dominated government in Lebanon strengthens this impression. Even more suggestive is the fact that hostilities among rival factions resumed the moment Israeli forces withdrew from a certain portion of Lebanon.[31]

The Israeli government maintained during the Lebanon War that all that Israel wanted was some peace and quiet along its northern border, which could only be accomplished if the P.L.O. (and the Syrians) were driven out of the country. But driving them out was not enough. There had to be a strong central government to make sure they stayed out. Whether or not Israel wanted it, the Christians were the only faction with a strong interest in keeping the P.L.O. and the Syrians out. The Israeli

[31] A discussion of the calculations made by the various parties in Lebanon is in Maoz and Felsenthal (fn. 6), 193-96.

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government may have had little choice in its selection of allies. Even if it did not intend to impose a Christian-controlled government in Lebanon, other factions behaved as if it did. In short, Israel may not have had a choice of alternatives in controlling actors or resources in the Lebanese case.

There is another, more subtle, aspect to the rational explanation of the power paradox in the case of Lebanon. The P.L.O. in that country, although far from a military threat of strategic significance, posed a major political threat to the right-wing Israeli government of Menachem Begin. In July 1981, Israel had to sign—pretty much against Begin's and Sharon's best judgment—an indirect cease-fire agreement with the P.L.O., which the latter did its utmost to preserve despite repeated Israeli provocations. With the final phase of Israeli withdrawal from the Sinai scheduled for March of 1982, the Israeli government had every reason to expect strong political pressure from the United States for concessions. The self-imposed restraint of the P.L.O. in southern Lebanon indicates that the Reagan administration, which had been instrumental in arranging the cease-fire, might be attempting to get the P.L.O. into the negotiations on the future of the West Bank. Seen in these terms, a military blow to the P.L.O. must have become a politically attractive option even though a possible entanglement in Lebanese politics was anticipated. Thus, the paradox of power the Israelis experienced in Lebanon can be seen as part of a larger strategic context in which not making an attempt to increase Israel's ability to control the behavior of other actors (in this case, the Christian Phalanges) was rationally inadmissible to the Israeli government.

The problem with Israel's invasion of Lebanon was that it tried to accomplish too much. The Israelis applied a lot of resources because they wanted to control a lot of outcomes. But at least some of the goals they set for themselves (especially that of driving the Syrians out) were not necessary for the attainment of the main war aim of driving the P.L.O. out of Lebanon. It is quite plausible that it was precisely the tendency to accomplish everything that caused Israel to accomplish nothing. In any case, some analysts have argued that Israel may have had little choice but to confront the Syrians.[32] The Syrian anti-aircraft missile batteries in the Bekaa Valley would have endangered Israeli aerial operations in Lebanon.

The cognitive interpretation of Israel's self-made trap focuses on the nature of the predictions the Israelis made about the other actors' strategy

[32] See, for example, Avner Yaniv and Robert Lieber, "Personal Whim or Strategic Imperative: The Israeli Invasion of Lebanon," *International Security* 8 (June 1983), 117-42.

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choices, given the shared knowledge of Israel's military superiority. Since the Israeli government correctly perceived the situation as one in which everybody knew that it had the capacity to invade Lebanon and to drive the P.L.O. and the Syrians out, and that this was indeed its preferred policy, it assumed that the game would be over before it started, and that, once Syria's control over Lebanon was broken, the other actors would continue their old all-against-all struggle. This would have allowed the Israelis to impose a Christian-controlled government with which everybody else would have little choice but to cooperate. Thus, a simple extrapolation from a shared knowledge of the distribution of capabilities grew into a belief that a divide-and-rule policy would accomplish peace and stability in Lebanon. But this extrapolation ignored the threat such a policy imposed on the other weak actors in Lebanon (e.g., the Druzes and the Shi'ite Moslems). Because the other political factions in Lebanon did not like the prospect of an Israeli-Christian condominium, they started working together to disrupt it.

VI. Conclusion

Werner Leinfellner describes paradoxes as "unsolvable problems for rational logicians."[33] The present study has attempted to demonstrate that the paradox of power and conflict outcomes fits this definition. Expectations of a monotonically increasing functional relationship between control over resources or over actors on the one hand, and control over outcomes on the other, are in most cases quite reasonable and quite in accord with reality. This relationship is not as straightforward as one might expect, however. The main theme of this study has been that extra control over resources or over the behavior of other actors can *cause* a deterioration in the ability to accomplish desired outcomes or to prevent the occurrence of undesired ones. This is the essence of the paradox of power and conflict outcomes, because force is not used for its own sake in international politics. States initiate, or get involved in, violent international confrontations because they believe that such strategies will improve their ability to control outcomes. Even when they are not involved in actual confrontations, the dynamics of military growth and the resources allocated to defense are predicated on this logic.

The problem we have attempted to delineate is that this logic may in some cases be a crucial factor in causing the state to lose control over outcomes. It is important to note that it is not a Prisoners' Dilemma problem;

[33] Werner Leinfellner, "The Prisoner's Dilemma and Its Evolutionary Iteration," in Diekman and Mitter (fn.6), 135.

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in the PD game, *both* actors are done in by rational logic. Here the paradox is that increased control over resources hurts some actors, while decreased control over resources may well benefit others. Previous research on the determinants of success in interstate disputes has found that, when actors who control resources are also the ones that possess other motivational attributes such as high expected utility for conflict, high willingness to suffer, or a high level of displayed resolve, they tend to do very well in their violent or potentially violent interactions with opponents. Yet, when control over resources does not match the actors' willingness to incur the costs associated with the management of international conflicts or the risks associated with their potential escalation, they tend to perform poorly in such competitions.[34]

My argument is that it is not the acquisition of extra resources that, in itself, invokes tacit collusion among inferior actors. *Rather, it is the mix of extra resources and the known preferences of the superior actor that make for a fatal combination. Inferior actors act to offset extra resources, not because they feel directly threatened by the resources, but because they do not like the way these resources are going to be used, given the preferences of the superior actor.* When the Israeli forces marched into the Shi'ite villages in southern Lebanon and into the Druze villages in the Shuff mountains, they were welcomed with rice by the local population. What harmed the Israelis was that they sought a policy that threatened the other factions in Lebanon. The practice of throwing rice on the Israeli forces was replaced by that of throwing Molotov cocktails at them. When the Bulgarian troops had difficulties with the Adrianople siege in the final phase of the First Balkan War, both Serbia and Greece dispatched forces to help them. What harmed the Bulgarians was not the extra resources that had been gained in the first war, but their unwillingness to share these with their former allies in a more equitable fashion.

The process of tacit or formal collusion by inferior actors against superior ones may suggest that the thesis presented above is nothing but a somewhat different formulation of the old balance-of-power argument. Indeed, this criticism is not without basis. Previous analyses of the chairman paradox have pointed out that balancing processes in the international system take a form similar to the tacit collusion process suggested by that paradox.[35] Despite these balancing aspects, however, this study is

[34] See especially Bueno de Mesquita (fn. 3), on expected utility and conflict outcomes; Maoz (fn. 12, 1983), on resolve; Steven Rosen, "War Power and the Willingness to Suffer," in Russett (fn.3), on willpower; and David A. Baldwin, "Power Analysis and World Politics: New Trends versus Old Tendencies," *World Politics* 31 (January 1979), 161-94, on suitability of resources to political and military objectives.

[35] Brams *et al.* (fn. 6), 253; Maoz and Felsenthal (fn. 6), 196-97.

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really focused on the unit level of analysis, because the problem—as I see it—is primarily one of a choice among different levels of resources. Three aspects of the paradox suggest that a balance-of-power interpretation might be less than satisfactory in this case.

First, the paradoxes are driven by preferences and strategy, not by an automatic mechanism that prevents preponderance. The theoretical demonstrations of the paradox suggest that the actual occurrence of an inverse association between control over resources and control over outcomes is not always determined by how resources are distributed over the system of actors, but by how actors' preferences are distributed over a given array of outcomes. Acquisition of resources or prerogatives by an actor may or may not result in a paradoxical outcome, depending on the preferences of the actor and the strategies it pursues.[36] This suggests that a perspective focusing on the effects of structures—defined in terms of distribution of capability—on outcomes may provide only a partial account of the paradox.[37]

Second, from a unit-level perspective, the implication of power paradoxes is that—whether or not a balancing act is what accounts for the discrepancy between resources and outcomes—the focal actor gets hurt, sometimes badly. This raises the question of whether a more judicious allocation of resources by one actor can prevent the balancing act of other actors. When one is aware of the possibly adverse consequences of extra resources, there is a motive to cut down. The rational explanation argues that in some cases, actors cannot afford to do that. In this sense, a systemic perspective is useful in that it highlights the constraints on optimal strategy selection by actors. Even then, however, actors can sometimes manipulate the rules produced by systemic structures in a manner that allows them to have their cake and eat it too. Self-binding commitments on the part of superior actors may produce favorable results that re-establish the

[36] Steven J. Brams, Dan S. Felsenthal, and Zeev Maoz, "Chairman Paradoxes Under Approval Voting," in Gerald Eberlein and Hal Berghel, eds., *Theory and Decision* (Dordrecht, Holland: D. Riedel Publishing Company, 1987), 223-33, show that chairman paradoxes are independent of the choice procedure or the distribution of resources. Felsenthal and Maoz (fn. 6) show the same thing with regard to the monotonicity paradox.

[37] The extent to which a systemic perspective such as Waltz's provides a solid context to an analysis of power paradoxes depends on which of Waltz's arguments is taken as characteristic of a systems theory of international politics. If it is that "structures cause actions to have consequences they were not intended to have," as he contends in *Theory of International Politics* (Reading, MA: Addison Wesley, 1979), 107, then I do not agree that this argument is applicable to the power paradox. However, if it is the assertion he makes in "Reflections on Theory of International Politics: Response to My Critics," in Robert O. Keohane, ed., *Neorealism and Its Critics* (New York: Columbia University Press, 1986), 338, that "structure is not independent of the parts, the states as actors, but constantly interacts with them," then such a perspective seems a reasonable theoretical context within which the rational explanation can be cast.

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positive relationship between resources and outcomes.[38] When actors make choices that are based on fundamental misperceptions about the implications of extra resources and about the relations between resources and outcomes, however, the balancing act and other system-level explanations have little to offer. The actors are done in by their rash selection of resources. A structure that cannot be destroyed by bad choices or a structure that cannot be manipulated by smart strategies has yet to be invented.

Finally, both the theoretical demonstration of the power paradoxes and the empirical cases suggest that a paradox of power may arise even in the absence of communication or coalition formation among inferior actors. Noncooperative game theory is a useful theoretical basis for this paradox because it shows that all it takes to defeat an actor with superior resources is strategic behavior on the part of all individual participants. The notion of tacit collusion has no operational meaning under the assumption of noncooperation employed in the theoretical demonstration of the paradox. It merely represents a process wherein acquisition of resources by one actor produces independent strategy changes by other actors in a manner that directly harms the first and may or may not benefit the latter. In the case of Lebanon, there was no formal process of alliance formation among the various factions that fought Israel; each did it pretty much on its own.

I have not attempted to determine which of the two explanations of the power paradox is better. In some ways they complement one another. An actor may start accumulating resources without realizing that it might be harmed eventually. Once a pattern of resource accumulation is established, the paradox may emerge even if the focal actor becomes aware of the adverse implications of extra resources, and even if it decides to give up resources. Other actors may proceed in a manner that harms the focal actor because they do not believe that the former is willing to relinquish resources.

This suggests yet another paradox: the paradox of learning. The cognitive explanation implies that the choice of resources may become more rational, and hence more beneficial, once we incorporate into our theories the notion that control over additional resources opens to other actors an array of strategies that would otherwise have been inadmissible, and that the employment of these strategies might do more harm than the strategies employed by actors when the focal actor controls fewer resources.

[38] Maoz and Felsenthal (fn. 6) discuss how a chairman can resolve the paradox without relinquishing his prerogative: by a credible self-binding commitment to break ties strategically for less preferred alternatives rather than for more preferred ones.

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The resolution of power paradoxes therefore seems to be a straightforward consequence of our ability to use theoretical knowledge in a rational fashion. Rationality provides the key for the resolution of power paradoxes. Yet, although this prescription is appealing from the cognitive perspective, it is quite misleading. Rationality does not offer a quick and easy solution: being rational does not mean having an easier time resolving the paradoxes of power.

What do the paradoxes of power imply for the way we study, teach, and practice international politics? I believe that the study of paradoxes of war in general, and the study of power paradoxes in particular, entail three important lessons for the theory and practice of international politics. The first lesson is that a theoretical system that seems logically solid may sometimes generate unexpected predictions. An assumption of straightforward behavioral expectations stemming from apparently solid theoretical systems may lead one into traps that a more thorough investigation might have avoided. Leinfellner states that social paradoxes are fascinating philosophical problems because:

> (1) . . . they arise from implicitly given, hidden dilemmas in the presuppositions and foundations of [theoretical] systems; (2) most of them are immune to rational methods for solving the contradictory statements they generate; (3) because of the contradictions they generate, they make the whole system worthless and are thus a threat to the whole system; (4) in paradoxes, the opposing views are always somehow related or interconnected.[39]

If the paradoxes of power discussed in this paper have anything to offer in the context of the theory and practice of international politics, it is that some of the major premises of the realist paradigm must be reexamined. The threat to this theory is not in its overly narrow assumptions or their empirical plausibility, as subscribers to complex interdependence paradigms would have us believe.[40] The key issue is not that power varies over issue areas. Rather, the rational explanation of power paradoxes suggests that a more thorough investigation of political realism must explore the intricate relationship between capabilities and strategy. We must therefore incorporate components of actor preferences and the strategies that such preferences imply into our models and empirical analyses. We must

[39] Leinfellner (fn. 33), 135-36.

[40] See, for example, Robert O. Keohane and Joseph S. Nye, Jr., *Power and Interdependence: World Politics in Transition* (Boston: Little, Brown, 1977); Richard W. Mansbach and John Vasquez, *In Search of Theory: A New Paradigm for Global Politics* (New York: Columbia University Press, 1981); John Vasquez, *The Power of Power Politics: A Critique* (New Brunswick, NJ: Rutgers University Press, 1983); and various authors in Keohane (fn. 37).

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also examine how the rules of play may affect the relationship between control over resources and control over outcomes.

The second lesson is, for me and probably for others who are committed to quantitative empirical investigations of international politics, the most telling among the three: trend data and substantive findings based on frequency counts are, in general, important guides to public policy making. But in making major policy decisions, these findings should not be taken for granted; the costs of ignoring the atypical cases may be severe. Statistical models of international processes are aimed at identifying modalities. If most of the empirical cases analyzed cluster neatly about the regression line or fall into the right cells in our contingency tables, our hypotheses are taken to have been supported by the data. At that point, as Singer puts it, "the more rigorous type [of scholar] permits himself that moment of pleasure for basking in the warmth of private discovery, and then gets on with the job of publicly visible, explicit, reproducible authentication."[41] The danger lies in being so persuaded by this "private discovery" based on a majority of cases that seem to fit our models as to ignore the ramifications of the atypical cases that do not fit. But it is quite possible that those atypical cases, however low their empirical frequency, pose a severe threat to the entire model that has allegedly been supported by vast amounts of data. The lesson is therefore that the study of exceptions is of no less importance than the uncovering of modalities, especially if we are to have any hope of making policy recommendations that really count.

There is a good reason for the use of a relatively stringent definition of paradoxes. Studies of paradoxes of power in international politics have employed an overly loose notion of the concept. For many students of international politics, a paradox of power simply means that "when the apparently weaker state wins . . . [international] conflicts [against a stronger state], analysts of international politics who rely heavily on 'power' as an explanatory concept face a paradox that creates doubts of the validity and utility of it."[42] As a result, many of the "solutions" of these "paradoxes" have been overly simplistic. For example, Organski and Kugler's analysis centers on the idea that the power disparity between the winner and the loser is more apparent than real, and that, if we measure national power correctly, we will find out that the winner of the "paradoxical" war was actually stronger than the loser. Therefore, the paradox of power seems

[41] J. David Singer, "The Incompleat Theorist: Insight Without Evidence," in Klaus Knorr and James N. Rosenau, eds., *Contending Approaches to International Relations* (Princeton: Princeton University Press, 1969), 65-66.

[42] James Lee Ray and Ayase Vural, "Power Disparities and Paradoxical Conflict Outcomes," *International Interactions* 12 (June 1986), 316.

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to be illusionary.[43] Ray and Vural wind up with a similar conclusion. Investigations that focus on factors other than control over resources (such as resolve, bargaining power, will to suffer, expected utility, and the like) contribute to the confusion. (I am including my own studies of resolve in this category.)[44] The common denominator of all these studies is that, having defined the paradox of power in noncausal terms, they downgrade its theoretical significance and practical implications.

The third lesson is that social paradoxes that have empirical referents would normally be illustrated by puzzling empirical observations that seem to defy intuitive logic. But the converse is not necessarily true. Puzzling empirical observations do not necessarily constitute paradoxes. Only that subset of puzzling observations that reflects a causally induced contradiction between premises of an otherwise logical system is a true paradox. Although the explanations of the David-and-Goliath phenomenon in international conflict may contain some grain of truth, they do not necessarily shed light on paradoxical conflict outcomes. What they attempt to do is to downgrade or disregard the more severe penalties of resource accumulation in international politics. They try to restore linear logic to problems that defy such a form of reasoning. The power of an analysis of paradoxes is that it explores inherent weaknesses in popular theories of international policies. But, in order to make use of the insights of such an analysis, the study of social paradoxes must be deductive in nature. A paradox must be demonstrated in the abstract, general case as a problem in rational logic. Empirical examinations of paradoxical conflict outcomes make substantive sense only if this has been accomplished.

[43] Organski and Kugler (fn. 3); see also Jacek Kugler and William Domke, "Comparing the Strength of Nations," *Comparative Political Studies* 19 (March 1986), 39-70.

[44] Ray and Vural (fn. 42), and James Lee Ray, *Global Politics*, 3rd ed. (Boston: Houghton Mifflin, 1987), 164-77, offer a comprehensive review of this literature.

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REFORMING A SOCIALIST STATE: Ideology and Public Finance in Yugoslavia

By SUSAN L. WOODWARD

Ivo Banac, *The National Question in Yugoslavia: Origins, History, Politics*. Ithaca, NY, and London: Cornell University Press, 1984, 452 pp.
Steven L. Burg, *Conflict and Cohesion in Socialist Yugoslavia: Political Decision Making since 1966*. Princeton: Princeton University Press, 1983, 377 pp.
April Carter, *Democratic Reform in Yugoslavia: The Changing Role of the Party*. Princeton: Princeton University Press, 1982, 285 pp.
Lenard Cohen and Paul Warwick, *Political Cohesion in a Fragile Mosaic: The Yugoslav Experience*. Boulder, CO: Westview Press, 1983, 213 pp.
Saul Estrin, *Self-management: Economic Theory and Yugoslav Practice*. Cambridge: Cambridge University Press, 1983, 276 pp.
Pedro Ramet, *Nationalism and Federalism in Yugoslavia, 1963-1983*. Bloomington: Indiana University Press, 1984, 317 pp.

Introduction

STUDIES of the state and its institutions remain confined within one of two analytical paradigms—the Marxian or the Weberian. Either one chooses a class analysis of political power and asks whether the state can act autonomously from capital, or one looks at administrative structures and legal definitions of jurisdiction and status and asks whether the state can be effective in implementing its policies in (or against) society. The current revival of interest in the state is in fact only a manifestation of dissatisfaction with the Marxian paradigm. Despite the irony of its neo-Weberian critique in an age where liberalization, privatization, and the dismantling of the state in general are in vogue, there have been few forays into alternatives. The one exception—the return to a Hegelian paradigm (the creation of a civil society around market relations and independent of the administrative state)—is not an alternative, but a disguised compromise that pulls toward the pluralists' agnosticism.

The two paradigms could remain separate and in opposition in large part for historical reasons. Marx analyzed the evolution of states that were under attack from the holders of mobile assets—"capital," elaborating the physiocratic critique. Weber analyzed states built for the military defense of landed assets and for binding the labor necessary to work and defend them. Class and status generalize different kinds of capital-labor

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relations and state-international boundaries, and therefore call for different institutional arrangements. This contrast is perhaps most clearly seen in their different notions of ideology. As long as the liberal project in the West proceeded and the statist project remained dominant in the East—the divide that Perry Anderson and Robert Brenner have done so much to document—this separation had some empirical justification.[1] It has been prolonged by the locus of recent studies of the state—in the 19th and early 20th centuries or in the equivalent state-building projects in industrializing ("developing") countries.[2] Nonetheless, the separation was always more analytically useful than empirically "true," as the debate over the characteristics of East Central European states,[3] or the difficulty of analyzing historical mixtures such as the French state, make clear.[4]

The need for some revision in our approach—rather than the periodic swings between Marx and Weber and their respective virtues and blindspots—is manifest when we look at the actual development of contemporary states: the administrative character of liberal states, the mixed economies and state forms in most of the world, and the further internationalization of strategic conflict as well as of markets.

One particularly inviting opportunity for such a revision lies with the economic and political reform processes in contemporary "state socialist" countries. There, rulers began with the institutions of Weberian states, but they transformed the content of those institutions in two ways: by placing in state offices a new political class representing labor and by assuming control over domestic capital as Marx defined it. The historical process by which economic and political power were being institutionally

[1] Perry Anderson, *Lineages of the Absolutist State* (London: Verso, 1979); Robert Brenner, "Agrarian Class Structure and Economic Development in Pre-Industrial Europe," *Past and Present* (No. 70, 1976), 30-75.

[2] This is true of the most representative of neoinstitutionalist works, Peter B. Evans, Dietrich Rueschemeyer, and Theda Skocpol, eds., *Bringing the State Back In* (Cambridge: Cambridge University Press, 1985), and of two primary practitioners—Theda Skocpol in her work on the New Deal and in *States and Social Revolutions: A Comparative Analysis of France, Russia and China* (Cambridge: Cambridge University Press, 1979), and Stephen Skowronek, *Building a New American State: The Expansion of National Administrative Capacities 1877-1920* (Cambridge: Cambridge University Press, 1982).

[3] See T. H. Aston and C.H.E. Philpin, *The Brenner Debate: Agrarian Class Structure and Economic Development in Pre-Industrial Europe* (Cambridge: Cambridge University Press, 1987); Andrew Janos, *The Politics of Backwardness in Hungary, 1825-1945* (Princeton: Princeton University Press, 1982); and the work of historians in Eastern Europe, especially the Hungarian Peter Hanak.

[4] Much of what Marx wrote about the "state" was after all developed in response to the French "mix," particularly in the 1840s-50s writings, *Class Struggles in France, 1848-50* and *The 18th Brumaire of Louis Bonaparte*; the alternation between regimes and the central-regional conflicts, particularly in the political economic alliances they represented, in 19th- and early 20th-century France have characteristics strikingly similar to contemporary Yugoslavia.

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distinguished (including private property rights and the political institutions of civil society such as class-based associations, electoral competition, and separation of powers) was reversed. The actual outcome has been mixed societies that are characterized by a mass of overlapping, shared, nonexclusive property rights and state privileges; by unending group struggles to gain institutionalized power or protection for their particular assets; and by institutions that combine military, trading, and welfare states.

Most analyses of these states are in the Weberian paradigm, but its inadequacies within such a reality are apparent. Thus, some studies incorporate the language of class alongside Weberian categories of status and hierarchy;[5] others argue that the two are conflicting tendencies within individual societies.[6] There is also the unresolved paradox of continuity and change. State structures and their ideology are represented as constant and even resistant to change; states alter only their relations with their environment—as in the concept of the social contract, where the state remains the same but it shifts clientele or redraws the boundary between state and civil society.[7] However, many of these studies actually describe frequent administrative reform, alteration of state rules, offices, and economic organization, and significant political competition. Other authors simply abandon the state and resort to pluralist models of its politics, but their findings are not then incorporated into models of the state itself.[8]

[5] The best of these is the work by Ivan Szelenyi; see, for example, "The Intelligentsia in the Class Structure of State-Socialist Societies," in Michael Burawoy and Theda Skocpol, eds., *Marxist Inquiries: Studies of Labor, Class, and States* (Chicago: University of Chicago Press, 1982), 287-326.

[6] Zygmunt Bauman, "Officialdom and Class: Bases of Inequality in Socialist Society," in Frank Parkin, ed., *The Social Analysis of Class Structure* (London: Tavistock, 1974), 129-48.

[7] The continuity of the Soviet state with Russian autocracy, of Bolshevism with the Christian church or with prerevolutionary absolutism, and the concept of Leninist regimes as an ideology of organization that is constant are examples; see Zbigniew Brzezinski, "The Patterns of Autocracy," in Cyril E. Black, ed., *The Transformation of Russian Society: Aspects of Social Change since 1861* (Cambridge: Harvard University Press, 1960), 93-110; Stephen F. Cohen, "Bolshevism and Stalinism," in Robert C. Tucker, ed., *Stalinism: Essays in Historical Interpretation* (New York: Norton, 1977), 3-19; and the work of Kenneth Jowitt, esp. "An Organizational Approach to the Study of Political Culture in Marxist-Leninist Systems," *American Political Science Review* 68 (September 1974), 1171-91, *The Leninist Response to National Dependency* (Berkeley: Institute for International Affairs, University of California, 1978), and "Inclusion and Mobilization in European Leninist Regimes," *World Politics* 28 (October 1975), 69-96. On the social contract argument, see Alex Pravda, "East-West Interdependence and the Social Compact in Eastern Europe," in Morris Bornstein et al., eds., *East-West Relations and the Future of Eastern Europe* (Boston: George Allen & Unwin, 1981), 162-187; Timothy Colton, *The Dilemma of Reform in the Soviet Union*, 2d ed. (New York: Council on Foreign Relations, 1986); and Peter Hauslohner, "Gorbachev's Social Contract," *Soviet Economy* 3 (No. 1, 1987), 54-89.

[8] This approach begins with H. Gordon Skilling, "Interest Groups and Communist Politics," *World Politics* 18 (April 1966), 435-51, and has been furthered most by Jerry Hough. Two fine recent contributions are Nina Halpern, "Policy Communities, Garbage Cans, and

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Reform complicates the study of socialist states with existing paradigms even more. They are moving from Weberian to liberal states, not through the force of bourgeois revolution, but with the guidance of party leaders with a Marxian project in mind. Moreover, they are themselves attempting to create civil societies independent of the administration. A reality so similar to, and yet so different from, the historical processes that define current paradigms of the state calls for an alternative approach.

Because such an inquiry is also a historical process, an evolution with many ebbs and flows, actual cases of reform are probably the best place to start. There is hardly a more suitable vehicle to study this process and the character of the resulting state than the case of Yugoslavia. Its economic and political reform of the state that Weber analyzed has been going on longer and more consistently than any other, including the rival reforms of the Soviet Union and Hungary. Yet, ironically, it is the consistency in Yugoslavia of a Leninist approach to the state that has led scholars to exclude it from the Leninist circle.[9] Because the Marxian project in the East took over from the earlier nationalist onslaught on the state, the continuity of institutional struggles and their ideologies that is so much a part of the literature on the Soviet Union (but, for obvious historical reasons, not on Hungary) is also pronounced in the case of Yugoslavia.

The reform of the Yugoslav state is the focus of the works under review. Their differences in explaining related (and even identical) changes illustrate the difficulties of the Weberian and pluralist paradigms—some combination of which all of them apply—in studying this state and its characteristic politics. Because they also provide richly descriptive and informative detail on reform politics, their disagreements can be used to carve away these difficulties and to get closer to what state institutions in these countries are all about. Three issues will receive attention in this review.

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the Chinese Economic Policy Process," unpub., presented at the annual meeting of the American Political Science Association, Chicago, August 1987, and Susan Gross Solomon, ed., *Pluralism in the Soviet Union* (New York: St. Martin's Press, 1983). A useful attempt to deal directly with this problem is Prasenjit Duara's concept of state involution, a "variation of the state-making process wherein the formal structures of the state grow simultaneously with informal structures"; see his "State Involution: A Study of Local Finances in North China, 1911-1935," *Comparative Studies in Society and History* 29 (January 1987), 132-61.

[9] This is the implication of Comisso's "State Structures and Political Processes outside the CMEA: A Comparison," in Ellen Comisso and Laura D'Andrea Tyson, eds., *Power, Purpose, and Collective Choice: Economic Strategy in Socialist States* (Ithaca, NY: Cornell University Press, 1986), 400-21. The debate between A. Ross Johnson and William Zimmerman originates in different views of the state itself, which can be seen in their discussion of Yugoslav international alliances; for Johnson, the state boundary is military and foreign policy; for Zimmerman, it is foreign trade. See Johnson, "Is Yugoslavia Leninist?" and Zimmerman, "Rejoinder," in *Studies in Comparative Communism* 10 (Winter 1977), 403-11. The Soviet reforms under Gorbachev make this Leninism of Yugoslav reform clearer.

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The first is the need to distinguish legitimizing ideologies from state institutions—ideology in both the Marxian sense of class hegemony and the Weberian sense of relations between rulers and their administrators, because both senses are present. Such a distinction helps to identify the elements of persistence and the role of ideologies in specific situations. The second is the need to take seriously the content of institutions—the social relations they formalize and the purposes they serve. Moreover, if the focus shifts from rhetoric about decentralization, autonomy, and democracy to the distribution of specific rights and privileges to employment and capital (whether status and taxation or property and profit) and the regulation of national-international and public-private boundaries, we can bring these states back into the world of other states. The third is the need to specify more directly the connection between pluralist pressures for institutional change and the specific content of proposals. Here, the Weberian perspective is not always used to its advantage. The following discussion will illustrate these issues from studies on the middle period of Yugoslav reforms.

Reforming the Yugoslav State

Between 1963 and 1974, the Yugoslav party leadership instigated major changes in the organizational structure and procedures for political recruitment and decision making of both ruling party and government. Although their proposals led to intense debate, the changes were codified in a new constitution in 1963, a new party statue in 1964, 42 constitutional amendments between 1967 and 1971, another constitution in 1974, and a host of enabling legislation on judicial reform, press freedom, economic management, the electoral system, and parliamentary and executive reorganization.

The authors of the studies under review differ on how to treat these changes—whether as a systematic reform or a series of ad hoc measures; whether it should be called democratization, decentralization, or federalization; whether the aim was to "separate the party from power" (Carter, 96ff.) or to reduce the power of central party organs over lower ones in the republics, provinces, and communes; they also differ on the dates of turning points. They agree, however, on the result: greater autonomy for political and social actors from the central dictate of executive party organs. The new rules served to separate policy-making authorities from operational administration; to reduce political coercion in enforcing obligations (for example, use of the security police or central party control over cadre and official appointments); to increase the activity of legisla-

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tures; to elect members of parliaments, government functionaries, and economic managers; to end the party's monopoly over economy and state with extensive pluralization; to encourage open debate and dissent within party and society; and to increase the autonomy of the republics in formulating their own and federal policy.

In *Democratic Reform in Yugoslavia*, April Carter argues that these changes began with concern over economic growth in the early 1960s and the adoption of a liberal program to improve efficiency. The program would increase the use of markets and the independence of enterprises and managers from governmental regulations and party controls so they could respond to price signals and market demand. Because this program led directly to a debate on the role the party should play in society and to calls for inner party democracy, Carter sees an unusual opportunity to test the "real possibilities and limits of democratic reform within a Communist party-state" (p. 1). Unlike the "arbitrary external check" in Eastern Europe, Yugoslavia's "independence from Moscow" left it free to pursue the path of democratization. The fate of the Czechoslovak reform movement implicitly haunts Carter's story; had she written a few years later, she might have chosen Gorbachev's reforms of 1987-1988 for an even more consequential test.

The answer, in Carter's view, depends on the outcome of factional conflict within the party over the political implications of reform institutions.[10] The reform faction originated in an alliance of liberals (managers and trade union leaders from profitable firms, local and republican party and government officials in the more developed republics) and of radical democrats (intellectuals and some rank-and-file unionists and students), who together opposed the power and privilege of the old-guard, partisan generation (especially veterans, security police, and local party bosses). In order to promote the younger generation and introduce professional standards for the selection and disciplining of officials, they proposed to decentralize the selection of cadres and to hold them accountable to electorates and legislatures from below rather than to those who appointed them from above; to cut the size of bureaucracies; and to forbid the accumulation of functions by party officials. The liberals then focused on the market reform, but the radicals pushed for participatory democracy and workers' control, reviving parts of the decentralizing program after the 1948 break with Stalin, which had been stopped by conservatives in 1953-1954 when they threatened the party's dominance.

[10] See Dorothy Solinger's triadic model in *Chinese Business under Socialism: The Politics of Domestic Commerce 1949-1980* (Berkeley and Los Angeles: University of California Press, 1985) for an account that more closely captures Yugoslav coalitions than the normal factional model of socialist state politics.

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The alliance collapsed under the strain of democratization: elections gave a "political underground" of party conservatives the means to regain some influence; workers mounted strikes and students demonstrated; and the radicals attacked the negative consequences of economic liberalization, such as income inequalities and unemployment. While the radicals intensified the struggle for legal guarantees to civil liberties, the liberals began a retreat on political reform. They replaced multicandidacy elections with rotation among positions, parliamentary debate with consultative committees behind closed doors, and the new diversity in party statutes, membership fees, and official salaries with reimposed central guidelines. They also reduced the scope of the freedoms of speech and assembly, and of the press and the judiciary, that had been granted. But the reform actually ended, Carter argues, when the liberals permitted the rise of competitive nationalisms that converted "claims to interest group autonomy and free speech into a direct political threat to the party's control" (p. 100). Persuaded that the radicals' program would lead to a multiparty system and that they themselves would not be able to control nationalist aspirations, the liberals allied with conservatives to recentralize the party and restore its orthodox role. Carter concludes, therefore, that all communist parties are unable to abandon Lenin's vanguard principle and to democratize.

Steven Burg, Pedro Ramet, and Lenard Cohen and Paul Warwick, in contrast, argue that the purpose of the institutional changes was to strengthen the party's rule against the threat that national conflicts pose to its legitimacy and to domestic political stability. The party claimed authority in 1943-1945 by promising national equality, institutionalized by a federal system; but this was a "temporary . . . symbolic gesture" based on the Marxist assumption that economic development would supplant national identities and ideologies with new social bonds (Cohen and Warwick, p. 141); Ramet even argues that "prior to 1963 Yugoslavia was a centralized state with a federal veneer" (p. 89). Because economic inequalities among the nations persisted, however, conflicts among the leaders of the republics intensified in the early 1960s; by 1966 they had forced an accounting on the real character of the state. In Burg's view, the state was a continuation of Serb hegemony no different from the interwar Kingdom: an ideology of Yugoslavism (that south Slavs are one people) imposed against national aspirations for autonomy; the "suppression of inter-nationality conflict" (p. 82) by the coercive apparatus of police, army, and central party control and cadre commissions (all headed by Serbs); and a highly centralized state and hegemonic party.

The difficulty, according to Burg, is that "nationality and the relative levels of development of the national territories," the "two dominant

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cleavages" in Yugoslav society "since before the creation of the Yugoslav state," overlap (p. 8). The federal system institutionalizes both of these cleavages; leaders' disputes over issues of economic allocation and governmental redistribution become national quarrels and translate readily into jurisdictional conflicts. Leaders of the republics divide into two camps, liberals and conservatives: in the more developed northwest, they prefer a liberal economic program and "national rights" (regional power); in the less developed southeast, leaders favor federal redistribution.[11] The victory of the liberals in 1966 created new problems, however; the accompanying political reforms reduced the repression of nationality conflicts and liberated national sentiments and popular energies, while decentralization exposed republic leaders and federal functionaries to new constituencies—their local or regional electorates. The pressures from below had the consequence of increasing disagreements among leaders as well as mass expressions of national antagonisms.

In order to protect the reforms, party leaders—the liberal majority—began a course that continues to this day: to adjust decision-making rules and central organs when major conflicts arise as one might adjust prices to balance markets. Their aim was to obtain consensus among regional party and government leaders, to reduce the immobilism of too much pluralism, and to create an effective equilibrium between the need for central authority and the imperative of regional autonomies. Because the economic reform created a system that worsened developmental disparities, however, and the highly inefficient decision-making process of bargaining and consensus building lowers economic performance, the ability to achieve consensus and maintain stability was strained anew with each new set of rules. Nonetheless, Burg concludes that when the leaders chose, during the 1970s, to counterbalance local control and growing social disorder with institutions to strengthen central authority, they did restore order and legitimacy.

The pluralization that Carter calls democratization is seen by Burg, Ramet, and Cohen and Warwick as a process of federalization, of devolution of power from the center to the republics, and of ending Serbian domination. Its effect was a fundamental transformation in the role of the party and the organization of the government. In Burg's view, the state moved from central dominance to multiple centers of regional power, and from unity by coercion to consensus through bargaining over deci-

[11] This classification rises and falls on the placement of Serbia, which many erroneously assume to be less developed. A conservative is loosely conceived in all these works on the 1960s as a supporter of the status quo ante 1966 (as these authors portray it); the term could be more carefully defined.

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sion-making rules and economic differences. Ramet perceives a new "cooperative federalism"; Cohen and Warwick focus on the ideological shift from a Yugoslavist policy of amalgamation to institutions that would permit national pluralism while accommodating and "harmonizing" their differences.

Behind these contrasting interpretations of the Yugoslav reforms lie different theoretical preconceptions. Carter and Estrin see a relation between desired economic outcomes and the institutions that structure economic behavior; they imply that markets require autonomous decision making by producers and will work less well the more competition is inhibited by state regulation or monopolistic behavior.[12] Carter assumes further that markets require political pluralism and democratization because the two reforms occur together; still, her inquiry is not concerned with the compatibility of markets and communist party rule, but with the political reasons for the failure (in her view) of reform. Because Burg, Ramet, and Cohen and Warwick are concerned instead with the possibilities for political stability in multinational communities, their analysis of the reforms starts with the assumption that political stability depends on consensus. In their view, a society segmented by national antagonisms requires special institutions to resolve conflicts and create mutual agreement.

Burg takes the model of consociationalism that Lijphart ascribes to the Netherlands.[13] Equating the Yugoslav republics and provinces with Dutch "political-cultural blocs," or *zuilen*, he argues that the Communist Party has maintained its power and social stability by confining conflicts to bloc leaders and altering decision rules and forums to maintain elite concert. The Yugoslav leadership did not respond to increasing squabbles as that model predicts, however, but by devolving power to each republic and away from authoritarian central control. Burg therefore shifts his explanation to the party's substantive promise of national equality and its search for renewed legitimacy.

[12] This is the standard argument of economic reformers in socialist systems, best known through the work of Janos Kornai; see, e.g., his *Contradictions and Dilemmas: Studies on the Socialist Economy and Society* (Budapest: Corvina, 1983). The literature tends to speak in metaphors and ideologies of market economies rather than to elaborate the how and why of the institutional change that is considered necessary for their operation. Nor are the economic and political aspects of their demands for greater autonomy as separate as some authors imply. Situated between this argument and Carter's is Ellen Comisso's thesis of market (autonomy) vs. plan (center) dialectic, which she posits for Yugoslav self-management in *Workers' Control under Plan and Market* (New Haven and London: Yale University Press, 1979).

[13] Arend Lijphart, *The Politics of Accommodation: Pluralism and Democracy in the Netherlands*, 2d ed., rev. (Berkeley and Los Angeles: University of California Press, 1975), and *Democracy in Plural Societies: A Comparative Exploration* (New Haven: Yale University Press, 1977).

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Ramet prefers the analogy of the inter-state alliances of 19th-century Europe as they have been formalized in the systems theory of Morton Kaplan.[14] In this view, the republics are corporate actors pursuing self-interested coalition strategies to increase their power capabilities within the federal system. He agrees with Burg that the goal is elite concert, but differs on the substantive motivations of leaders, and therefore on the institutions that necessarily follow. For each republic, the pursuit of power depends on preventing the emergence of any one republic as a dominant actor; the central party and government alter the institutions that regulate their interactions in order to create and maintain a balance of regional power.

For Cohen and Warwick, political stability lies in societal consensus. In the mode of Charles Tilly's thesis about state building in the West, they assume that success goes to rulers who design political institutions that undercut the political salience of ethnic loyalties within the population and meld substate cultural communities into "one people."[15] By treating changes in rules of representation between 1920 and 1980—gerrymandered administrative and electoral districts, repression and authoritarian exclusion of certain political parties, power sharing or group domination of central policy institutions, and so forth—as different regime strategies for incorporation, these authors attempt to measure, with data on electoral behavior, the political cohesion achieved by different institutions.

Despite their theoretical arguments, however, it is not clear whether any of them believe that state institutions—including the ones they trace—actually influence conflict regulation. In all five studies, the primary regulator is the highly personalized method of leadership purge. Purges also set the moment of institutional reform. For all, it was the forced resignation of Alexander Ranković in July 1966 (on charges brought by the military counterintelligence service of spying on President Tito) that fundamentally changed the Yugoslav state. A member of the wartime leadership, Ranković was a Serb whose control of the levers of central party power—the internal security police, the cadre commission, and the veterans' association—maintained, in their view, conservative

[14] Kaplan, *System and Process in International Politics* (New York: John Wiley & Sons, 1957). Another of Ramet's arguments is that central planning cannot exist without agreement, and that, because the leadership could not obtain agreement among the republics, it had to decentralize; this does not fit the fact that the move away from central planning occurred in 1950-1952.

[15] Charles Tilly, ed., *The Formation of National States in Western Europe* (Princeton: Princeton University Press, 1975), 3-83. Cohen and Warwick could of course be applying unitarist Yugoslavism (*narodno jedinstvo*) instead; see Banac, throughout.

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and Serbian rule. His removal made it possible to proceed with the organizational reform of the party that was approved in 1964 (Carter), the economic reform announced in 1965 (Estrin), and the federalizing constitutional reforms of 1967-1971 (Burg, Ramet) by making possible a purge of the security police and by depriving those who were obstructing implementation of the reforms of their patron. Other significant changes in the state begin with President Tito's personal decision to shift support among leaders—his choice of the liberals on economic reform in 1962, his purge of liberal leaders from Croatia and Serbia in 1972—and with his death in early 1980.[16] Steven Burg, the author who focuses most directly on organizational rules and offices, even argues that the new central institutions that were created to revitalize party authority in the 1970s were in fact aimed at resolving the succession problem—to institutionalize Tito's charismatic authority, which Burg considers the only effective solution until then to the national question and to political stability.

If the institutions do have some substance and if their reform (as well as the current Soviet or ongoing Hungarian and Polish reforms) affects the character and operation of the state itself, then the use of interpretive models from Western European history is a strange route to take. The fact that the evolution of the state over the last two centuries has been significantly different there from that in Central and Eastern Europe would seem to discount their substance in advance. For the relation between economic liberalization and democratization, one might more fruitfully turn to Asia and Latin America. As for the national question, if any borrowing from other experience is appropriate to this quintessentially East European question, then other postimperial states would provide better parallels than Western Europe. These two arguments—about democratization and the national question—can be used, however, to examine the methodological obstacles to understanding institutional change in the Yugoslav state.

Institutional Ideologies and the National Question

The first obstacle is the identification of institutions with the ideologies that authorize their occupants' exercise of power and then create entire

[16] This tendency to periodize politics in communist party-states according to their leaders' tenure is the norm; Valerie Bunce takes it to the level of axiom in *Do New Leaders Make a Difference?* (Princeton: Princeton University Press, 1981); Cohen and Warwick also periodize the interwar period by administrations. The case of purges is particularly debatable; they may reflect leaders' calculations that dissident colleagues could call on a sizable popular following should they choose to continue opposition to the policy taken. This would make purges significant political events, if not causes of change; but the argument needs to be made.

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cultures that inform peoples' understanding of social reality. It can be illustrated particularly well with the second argument—namely, that the political reforms addressed the national question and especially the Serb-Croat conflict. Ivo Banac's study of the origins of the national question in Yugoslavia provides the substantive content necessary to return to Yugoslav experience and to identify the national differences that are being "accommodated" by current political reform. It is also an excellent starting point for those who want to study actual cultures of rule in contemporary Yugoslavia.

For the authors under review, nations are essentially bundles of sentiments—"primordial" divisions of "language, religion, culture, territory, and nationality" (Burg, 8); they predispose people to mutual conflict and are so slow to change that political institutions must do the changing if the power of personal dislike and suspicion are not to destroy the state (and the party). According to Banac, however, these cultural differences actually concern state institutions. National conflict is "the conflict between the protagonists of several antagonistic national ideologies, which expressed themselves in the struggle over the organization of the new state" (Banac, 214).[17] The legitimacy of national institutions depends on their historical authenticity, moreover, because each nation has its own institutional lineage. Institutions cannot be alternately served up, withdrawn, or otherwise changed in order to resolve conflicts among nations with different political lineages.

Banac argues that nations begin as states. They continue in periods without a state as a communal consciousness of their previous sovereignty, a political memory of their right to self-governance and of the institutions that represented that right. Whatever else they may share, such as a language to communicate this memory, nations are defined by these core political institutions which also keep the memory alive. Together, the two become a national ideology. In the movements of national revival during the 19th century, intellectuals, politicians, and national authorities claimed a right to political power and defined the community they professed to represent on the basis of these ideologies. Banac thus doubts the possibility of successful multinational states; he argues the inevitability of

[17] Further,

> Whereas the proponents of unitarism sought to obliterate all historically derived differences between the Serbs, Croats, and Slovenes by means of a strictly centralized state, the majority of Serbian political parties used centralism to further Serbian predominance. This meant that the partisans of a unitarist and a Great Serbian version of Yugoslavia were counterposed to the representatives of the non-Serb national movements, who either demanded a federated (or confederated) state structure or sought guarantees for their national aspirations outside the framework of Yugoslavia (Banac, 214-15).

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failure, as in the case of the first Yugoslavia, if the institutions chosen for such a state are centralized and the idea of political community is unitarist. The Yugoslav Kingdom remained illegitimate in the eyes of members of nations that, in 1919-1921, sought a federal constitution based on pluralism to guarantee their right to their national institutions. Banac implies that, if the Serb national ideology had not been assimilationist and its institutions expansionist, but liberal and integrationist like those of the Croats and the Slovenes, there would have been no national conflict. Because Serb institutions prevailed, that nation gained hegemony, but all subsequent contests were interpreted as national conflicts.

The Serb institutions originated with the autocephalus Orthodox church that memorialized the medieval Serb state and its tsar—a combination of civil, religious, and military leader. Because Ottoman rule granted it both civil and religious authority and reinforced the co-sovereignty of nation and supranational church with the dyarchy of patriarch and sultan, the Serb vernacular was fairly late in separating from Church Slavonic as a literary, national tongue. The equation of nation and church and the assimilationist criterion of religious conversion for nationality kept the ideology unified. Its elements were reinforced by the Serb fight for national affirmation in the 19th century—armed revolts against Ottoman rule, followed by a policy of territorial aggrandizement and statecraft that justified as a "national mission" the unification with Serbia of all lands where Serbs had settled, and the living institutions of army and monarch and their "Jacobinism."

In sharp contrast, the parliament (*Sabor*) and governor (*Ban*) of the medieval Croat state were retained without interruption under the Habsburgs, with whom its landholding nobility had negotiated internal self-governance. The Croat army, however, became a unit of the Habsburg forces, and so fought imperial encroachments upon national institutions with legitimist treatises on ancient prerogatives. The struggle against the church hierarchy for use of the vernacular in liturgy and literature came early in the modern era, creating a tension between nation and church that was similar to that of the Muslims of Bosnia, another national unit of a universalist religion. The empire divided Croat lands administratively, however; as a result, the parties of national affirmation in the 19th century had to standardize three different Croat dialects and literary traditions before they could reunify the "heteropolar Croat lands" (Banac, 75).

When parties emerged within these divisions, each with a separate strategy for territorial defense, a second ideological tension arose. In civil Croatia and Slavonia, liberals (Banac's term for the Illyrians and Yugo-

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slavists) revived the idea of Slavic reciprocity (or integration through linguistic unity and ethnic continuity) and sent cultural missions to other Slavs in the empire. Operating primarily in the area of the former military border, the Party of (state) Right argued for political independence from the Habsburgs through armed revolt. In Dalmatia, the National Council combined liberalism (integrationism) and states' rights.

The conflict between Serbs and Croats, according to Banac, is thus not one of culture, in the sense implied by treatises on the Habsburg/Ottoman schizophrenia of the Yugoslav mentality or on the orientation of Croats to Catholic Europe and of Serbs to the Orthodox East, but of ideologies of territorial rule. While Banac castigates Serb ideology, he also locates the source of the conflict in the region's history of migrations. As civilizations were not "territorially constituted" (p. 69), and the relation between peoples and territory was never congruent, the fact that these ideologies claimed territory and human loyalties on grounds that are mutually incompatible meant that there could be no inclusive solution. Rivalry was inevitable. The confusion was compounded in 1917, when Serb and Croat parties unified on the basis of Yugoslavism. This ideology of the Croat integralists did not mean the same to both nations, either in their definition of a political community or in the institutions appropriate for a multinational state. It became a tragedy in the central ridge of migration (large parts of Bosnia, Slavonia, Vojvodina, and the former military border where Serbs had fled Ottoman rule into Croat lands) when the electoral contests of nationally defined political parties metamorphosed into genocidal massacres between competing armies and faiths in the civil war of 1941-1944.[18]

The national ideologies of Banac's study echo in the portrayals of postwar politics of the other works. Both Burg and Ramet characterize the second Yugoslav state from 1945 until 1966 as one of "Serb hegemony" because, in their assessment, the state was highly centralized; the coercive apparatus of police, army, and the central party control and cadre commissions "suppressed" cultural expression and the national question; individual Serbs headed the state apparatus; and the regime fostered an ideology of Yugoslavism. They explain the federalization, liberalization, shift to parliamentary activism, and purge of the coercive state apparatus

[18] In the elections just prior to the declaration of the dictatorship in 1929, Cohen and Warwick (p. 35) find a ridge of increasing entropy (measure of average social uncertainty for event-sets, in this case choice of party and turnout in parliamentary elections), or fragmentation into political subsystems, running through the region of mixed population and administrative systems of the former imperial borderlands that parallels Coleman's findings for North-South division in the United States in 1860. See Stephen Coleman, *Measurement and Analysis of Political Systems: A Science of Social Behavior* (New York: Wiley-Interscience, 1975).

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in the 1960s as the success of an anti-Serb coalition forged to diminish Serb dominance and restore the legitimacy of state institutions. Cohen and Warwick measure political identity solely by ethnicity and incorporation—in terms of support for or against the king and a nonfederal constitution in the interwar period, and of turnout and ballot invalidation in the communist period.[19] In April Carter's portrayal of party factions, Croats are often synonymous with liberals and a preference for parliamentary institutions; she equates Serbs with conservatives and a penchant for executive, coercive institutions. It is therefore no wonder that she finds that nationalist antagonisms overwhelmed the conflict between liberals and conservatives, and that the radicals dropped out of the picture. Moreover, all the authors juxtapose unitarist, centralist, and assimilationist ideologies and institutions to pluralist, autonomist demands. Their analyses of institutional change suggest the persistence and power of ideology as much as the content of the institutions or the reason for their change.[20]

The arguments themselves and their factual basis provide reasons to question this relation, however. For example, if the conflict between Serbs and Croats concerns state institutions and claims to territorial self-governance, why did the same antagonism not split the states'-rights and integralist Croats? On what basis are we to understand the enduring conflicts between Habsburg Serbs and the rulers of independent Serbia (including disputes over language and secularization), or between Vojvodina and Serbia proper, if the Serb national ideology contains no internal tensions? If the cause of the national question after 1919-1921 lies in the rejection of a federal constitution, why did its adoption in 1943-1945 not

[19] That is not to deny the extraordinary methodological problems of such analysis (which Cohen and Warwick elaborate), or their persuasive argument that electoral data are an underutilized source of information about Yugoslav politics; it is only to raise questions about the inferences they draw because of their assumptions about the data themselves and their choice of a contentless measure to make intertemporal comparisons. (Their hypothesis is that entropy measures for different regions will become more continuous and "harmonious" over time if the political system is becoming more stable and internally cohesive.)

[20] Because data are often gathered for political purposes, empirical analysis is not always a sure way around ideologies unless the content of those ideologies is taken into account; that is one of the many values of Banac's study. For example, the census of 1921 on which Cohen and Warwick rely for ethnic composition "reveals a good deal about the official ideology, [but] is not particularly helpful as a statistical guide to the size of each national community" (Banac, 49ff.). Banac's discussion of the elections of the 1920s also casts doubt on a major assumption of their analysis: that, because the elections were held under a "liberal democratic regime" and were thus "free," they "provide the most accurate measurement of the natural propensities of the political system" (Cohen and Warwick, 5). Banac, too, can succumb, however; see his use of data on taxation from the political pamphlet by Rudolf Bičanić for the Croatian Peasant Party, which initiated a famous debate on national discrimination in economic policy under the Kingdom in 1938-40; see *Ekonomska Podloga Hrvatskog Pitanja* [The economic basis of the Croatian question] (Zagreb: Vladko Maček, 1938).

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remove the issue from the historical agenda? If the failure of a legitimate constitution was the cause of the state's weakness and vulnerability to foreign interference during the Kingdom, what explains that weakness and vulnerability under the communists?

If the army, the secret police, and the central party were Serb institutions, and Ranković's departure was essential to removing Serb dominance, why was the damning evidence against him (the veracity of which is highly doubtful) provided by a party unit within the army?[21] The top political leaders and state builders—Croats Tito and Bakarić, Slovenes Kardelj and Kidrič, Montenegrins Djilas and Vukmanović, and the many Croats who staffed the federal government until the decentralization to the republics in 1950-1952 might be surprised to learn that the state was dominated by Serbs during its first two decades. If the monarchic and military institutions were personified by anyone, it was Tito, a Croat. The Kingdom had discriminated against former Habsburg officers (primarily Croats), treating them more as erstwhile enemies than as new compatriots, but the campaign to monopolize the domestic use of coercion after 1945 was perhaps bloodiest in Serbia. (Both states, however, had unfortunate and prolonged difficulty disarming the untrusting population of Kosovo). In 1948-1949, the purge of alleged Cominform sympathizers removed Serbs and Montenegrins from the party and officialdom to a disproportionate extent.[22] The cultural program of Yugoslavism vacillated: it was revived with reform in 1958, withdrawn with it in 1964. Since economic particularism by the national leaderships (as it was called in the early years) and federal-republic conflicts plagued the state from its very beginning, it is not clear what is meant by the "suppression of national conflicts."

The military origin of these national states and of competitive elections also persists in the authors' binary, adversarial analyses of postwar political struggles. Burg tends to condense political cleavages to a duel between a group of more developed and less developed republics and provinces, or between center and region. Cohen and Warwick, hampered by

[21] Burg cites the report of the party commission set up to investigate Ranković's crimes (i.e., to legitimize the purge). There is substantial circumstantial evidence to be skeptical toward its claims, however, including the striking parallels with the party commissions to investigate leaders purged earlier (e.g., Hebrang and Žujović in 1948), and later statements from political leaders, including Djilas, in *Tito: The Story from Inside* (New York: Harcourt Brace Jovanovich, 1980), and *Rise and Fall* (New York: Harcourt Brace Jovanovich, 1985). Burg's extensive use of party documents is a great strength; but, because these documents are also political acts, they need interpretation.

[22] See Ivo Banac, "Yugoslav Cominformist Organizations and Insurgent Activity: 1948-1954," in Wayne S. Vucinich, ed., *At the Brink of War and Peace: The Tito-Stalin Split in a Historic Perspective* (New York: Brooklyn College Press, 1982), 239-52; see also Banac's forthcoming monograph on the Cominformists.

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the limits of their electoral data, rest with oppositional categories, as do Carter's factional fights. Analytically, Ramet has an alternative in the shifting coalitions among eight self-interested units; but he, too, collapses these into a binary line-up on issues without identifying why each republic or province took a particular position, what it was, how it is best explained by his argument on motivations, and whether actual policies might be complex compromises among positions on several issues.

The disjuncture between ideology and institutions during the period of anti-imperial struggle for national self-determination (when the state-building rivals of 1917-1921 were organized) allows us to identify an additional difficulty—that of keeping levels of analysis separate. Because national ideologies are concerned with territorially defined power, they combine three hierarchies of social relations: the defense of borders and internal integrity against external threat; the property rights that define and legitimate the particular form of extraction necessary to the state's political class; and the principles on which those subject to this extraction are included—the closure of a national community with categories of regulated status and with rights, duties, and loyalties.

For example, the deliberative and executive bodies of the Croat nation were those of a state reduced to the feudal privileges and military obligations of the landowning nobility in the Habsburg estate system; its ideology of sovereign rule through primary acquisition was aimed at the Habsburg dynasty in competition over fiscal rights to the same territory. When shifts in the configuration of international power gave rise to fiscal crises for the imperial government, it responded with internal reforms, moving from the resources of warfare and feudal dues on agrarian economies to state-led commercial and industrial development and administrative rationalization. The fiscal privileges of nations and the rights of their national institutions came under attack. This process, and the radical change in status groups it brought, varies in its timing and form among administrative divisions of the empire, adding to the differences among regionally defined communities. New groups (merchants, lawyers, teachers, journalists, priests)[23] appropriated the ideology that had served an estate system to defend home rule against centralizing public finance, against unilingual and individually based criteria for political rights and the new civil employments (German under Joseph II, Hungarian in its territories after the 1830s), and against internationally mobile

[23] Banac (p. 29n) calculates that the 1838-1843 membership of Matica Ilirska, the Croat patriotic and cultural organization, consisted of administrators and public servants (27%), clergy (22%), free professionals (primarily lawyers) (18%), craftsmen and merchants (14%), teachers (7%), and noblemen (11%).

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goods and labor markets. The distance between that ideology and the new state institutions became so great that the ideology could no longer perform its task; it splintered among numerous, competing Croat political parties.

The primary conflict of these parties occurred at the international level: whether to regain rights and defend territory through Slavic integration or military liberation, and within or outside the empire. As political parties using legitimist claims, they were dependent on armed powers to guarantee these claims. Each party remained remarkably steadfast in its choice of foreign patrons: dynastic supporters against Hungary versus antagonists of Austrian expansion, and willing allies with independent Serbia or not.

At the level of domestic legitimation, they divided the patrimony and then fought over who was the legitimate heir. The Party of (state) Right maintained that the right to govern Croat land belonged to those who defended it; because their ancient right of the nobility was now shared with the "popularly based standing armies ... the lower classes became the political people" (Banac, 86), the constituency to which the party appealed in the demobilized border. The National Party cited the "Croat rights of ancient municipal autonomy," but since these rights did not suit its members' interests in commercial and cultural expansion, it turned to Slavic reciprocity and a federation of regionally autonomous (no longer noble) parliaments.

Only at the third level, the definition of national membership, did Croat nationalists strive for unity through linguistic standardization and come into direct "ideological" conflict with Serb assimilationism. The debates over language read like electoral campaigns, however, as politicians hoping to wear the mantle of national legitimacy wrote and rewrote programs to gain a popular following and implant permanent loyalties against competitors. This game, too, was played within rules set by external powers: at first, these rules were the criterion for civil employment and obligations within the empire (against which both Serbs and Croats reacted with the idea of national communities); later, they were those of Great Power negotiators at Versailles aiming to break up the empire by means of national self-determination.[24] The fierce competition for national loyalties in some regions was due less to the migration of peoples than to the multiplicity of status groups that left political identity unsettled once the regulations dissolved. In the military border area governed

[24] Ethnographers, members of a still young profession, were included in national delegations to the negotiating tables to provide "scientific" evidence about the ethnic composition of territories; the most famous was Jovan Cvijić, who worked on behalf of the Serbs.

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by the war office until its dissolution in the 1870s, for example, the household *zadruga*[25] received land-use rights in exchange for guard duty and imperial military service; but these rights were withdrawn if it could not provide soldiers, and governing rights were reserved for officers and landowners. Marriage prohibitions across status lines reinforced this political hierarchy with social separation. The Ottoman millet system produced similar complexities in Bosnia, and communal competition for members erupted among Muslim landlords, Catholic priests, and Serb merchants when Habsburg control ended status prescriptions in 1878.[26]

If Banac is correct that legitimacy depends on historical authenticity—"how faithfully each national ideology reflects the great issues of a nation's history" (p. 29)[27]—then ideologies that carry memories of former rights can help to empower groups that have such an inheritance.[28] Groups that can successfully claim nationhood in contemporary Yugoslavia have a fundamental advantage over those that cannot. The continuing struggle by the Muslims of Bosnia and the Albanians of Kosovo to achieve national status and the right to a republic in the federation rather than the secondary (but recognized) status of "nationality" are obvious examples. The fact that the Croats, Serbs, and Slovenes formed national movements and then political parties (including separate communist parties) gave them a preponderant influence over other groups in shaping the second Yugoslav state. But the content of that ideological in-

[25] A legally defined unit, not kin-based like the extended household in Serbia of the same name and ethnographic fame. See Robert F. Byrnes, ed., *Communal Families in the Balkans: Essays by Philip E. Moseley and Essays in His Honor* (Notre Dame, IN: University of Notre Dame Press, 1976).

[26] See Robert J. Donia, *Islam under the Double Eagle: The Muslims of Bosnia and Hercegovina, 1878-1914* (Boulder, CO: East European Monographs, 1981). The situation in Dalmatia (which ended up under Habsburg control) shows yet another set of complexities, primarily because of earlier Venetian policies. Verdery's argument on Romania is analytically very helpful: she identifies the "social-structural foundations" of group difference, where ethnic "cultures" are associated with different status groups, often territorially defined, and how shifts in status led to shifts in ethnic identity. See Katherine Verdery, *Transylvanian Villagers: Three Centuries of Political, Economic, and Ethnic Change* (Berkeley and Los Angeles: University of California Press, 1983).

[27] This is why, according to Banac, intellectuals create nationalist movements. Moreover, they remain a political force in this part of the world—and are perceived as a threat by ruling groups—because of their ability to keep the memory of previous political rights alive and to interpret that tradition under changing conditions. There is a similarity with the "literature of memory" tradition in Central and Eastern Europe, which should prove the usefulness of Banac's discussion of the South Slav case to other national movements in the area, such as the Poles and the Czechs, especially those originating within the Habsburg Empire. A contrasting approach is in Benedict Anderson, *Imagined Communities: Reflections on the Origin and Spread of Nationalism* (London: Verso, 1983).

[28] Because the farming class in Sweden, for example, had its own estate separate from the nobility, it could organize independently; this influenced the alliances and policies possible in Sweden's 20th-century state. For an elaboration, see Gøsta Esping-Andersen, *Politics Against Markets: The Social Democratic Road to Power* (Princeton: Princeton University Press, 1985).

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heritance matters also: tensions between Croat ideology and changed conditions, for example, produced as great a source of difficulty as did Serb ideology.

Reliance on an ideology that granted fiscal, property, and governing privileges over land to a single class/status in a liberal democratic age created lasting contradictions for Croat political action. The separation of political and economic rights, their frequent conflict, the importance of mobile as well as land-based assets, and the increasingly universal criterion for politically defined rights that made them open to all laid the basis for competing political parties. No matter how many internal tensions it contains, a national identity bound to a corporate definition of economic and political rights cannot sustain that plurality. Only one movement can be the legitimate heir to rights that are indivisible. The contrast between Croat history, on the one hand, and the Slovene and Serb histories, on the other, is instructive.[29]

Both of the latter lost "noble" status and its institutions (army, parliament) under imperial rule, but retained local self-governance. The national ideologies, like the fates of the Slovenes and Serbs, reflected their individual, unpropertied status. Mobility into the political or propertied classes was not incompatible with national identity (through military valor or economic success in Slovenia, or the *devshirme* whereby Serb children were taken into the Ottoman elite army corps or imperial administration as slaves and converted to Islam). The Theresian reforms encountered the nobles' reaction in Croatia-Slavonia, but gave financial autonomy to Slovene localities, who used it to build schools and roads. Local control and occupational and geographical mobility of educated citizens and migratory artisans remained compatible. Serbian independence was accompanied by policies to rebind peasants to the land, with homesteading rights and restraints on urban-rural trade. Until constitutional liberals gained the majority in parliament, significant resources were devoted to the creation of an army and engineering corps, but the ideology permitted independent political organizations along class, policy, and other nonterritorial lines within the nation.

Not burdened by an historicist ideology, both nations could adopt "natural right" ideas from the Enlightenment—such as the Slovene "natural right of national liberty based on linguistic individuality" (Banac, 112). The Slovenes used their linguistic separateness as a defense; the Serbs used their army and statecraft. Although Banac calls the first liberal

[29] Further illustration of such difficulties is found in Banac's fascinating discussion (pp. 359-78) of the quandaries of the Muslim political organizations (above all the JMO) during the interwar period.

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and the second expansionist, the fact is that both could be liberal without being nationalist. Serbs and Slovenes were able to ally in both the first and the second Yugoslav state without losing national identity. Croat liberals, however, seemed bound to their national demands, thus apparently making it impossible for liberals from all three nations to find common ground. As nations, moreover, Slovenes and Serbs could legitimately pursue coalition strategies that were either national and protective or cross-national and expansionist.

The circumstances of south Slav unification highlight these separate levels of power as well as the political contradictions of Croat ideology under modern conditions. Years of ideological debate ended abruptly in 1915 when the Serbian army and the Yugoslav Committee of Dalmatia signed the Corfu Declaration to prevent further loss of territory.[30] After Austria fell and the national guard was unable to protect landed and industrial property at home, the agreement was joined by the remaining national councils of Habsburg Slavs. Fearing popular radicalization along the Bolshevik example, these national representatives perceived the Serb army as their institution of "salvation" under the circumstances.[31] But the financial policies necessary to repay the war debts levied on Serbia by the Great Powers, on top of those necessary to impose order militarily and ensure the army's monopoly over armed might, turned this temporary convergence of national and propertied interests into mutual antagonism. Nationally defined political parties used national ideologies to voice perceptions of discrimination.

The fact that other ideologies did not supplant national ones was due in large part to the single voice of Croat objections. The Croat People's Peasant Party (CPPP) under its radical helmsman, Stjepan Radić, "synthesize[d] the previous Croat national ideologies in a new mass-based movement that completed the shaping of modern Croat nationhood during the interwar period" after Radić abandoned unitarist Yugoslavism in 1918 (Banac, 105). Banac does not pursue the irony of a peasant party defending landed, commercial, and industrial property, or of continuing to identify the army as a Serb national institution and the Serb king as the personification of a hated regime because of the corvée, conscription, and the branding of draft animals—even though the state used the military

[30] The Allies offered much of Dalmatia to Italy if it would shift sides in the war; the Habsburg threat of a separate peace included an independent Croatia.

[31] Banac (p. 131) quotes a report by a National Council official in Slavonia (Croatia):

> The people are in revolt. Total disorganization prevails. Only the army, moreover only the Serbian army, can restore order. The people are burning and destroying. . . . The mob is now pillaging the merchants, since all the landed estates have already been destroyed. Private fortunes are destroyed. The Serbian army is the only salvation.

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and gendarmerie (disproportionately staffed with Serbs) to "safeguard landlord properties" and crush what it called, in a single breath, "republican, socialist, and anti-Serb" dissent (p. 147). That Radić could lead both the opposition and the Croat public, despite his program of republicanism, pacifism, populism, and radical redistribution, can only be explained by the packaging of state policies and party programs in national covers, and by Radić's federal demands for autonomy to "historical provinces" and their parliaments. The compromise of 1939 between national foes, which did give Croatia autonomy, was negotiated by a Croat Peasant Party (CPP—the "People's" no longer) whose leading elements were now thoroughly urban and propertied, and a central government desperate to relieve the fiscal strains of war mobilization.

Until the 1930s, the nationalized quarrel prevented the integration of all those legal, administrative, and economic institutions that make a state. Reality was different from the unitarist, centralist ideology. The corruption attributed to Serbs, armies, kings, and bureaucracies had its source in the state's inability to secure sufficient revenues through legitimate channels. Bureaucrats and soldiers had to supplement their salaries privately. The government paid with land for military service. The overweening influence of foreign states on domestic politics and institutions came by way of the foreign loans and investments that the state sought to supplement the royal budget.[32] The fact that state expenditures favored defense, foreign affairs, education, and public order, and therefore individuals in those occupations, was more a measure of the government's insufficient capacity and the lack of agreement on any other basis of common interests than of the dominance of Serb ideology.

The electoral system added to the contradiction between national perceptions and daily reality. Pressured by the Versailles powers into a liberal democratic constitution, party leaders had to win country-wide numerical victories. This raised the ante of the unitary versus federal choice: the Serbs, though not a majority, were most numerous if they voted by national identification rather than by residence or property status; in the face of the CPPP's strength, they were motivated to play the national game. The rules also reinvited ideological conflict over national identity, giving an edge to assimilationist definitions, fostering anti-Serbism as the basis for opposition coalitions, and prolonging the packaging of multiple

[32] Only when pressure by the British and French for liberal democratic institutions yielded in 1929 was the king relieved of any restraint on outright dictatorship. Four years later, to appease German militarism, Britain and France handed Yugoslavia, along with the rest of Eastern Europe, over to a German sphere of interest; the king was assassinated by Croat and Macedonian fascists paid by Mussolini; and the domestic political order under Premier Stojadinović took on ever more coercive tendencies and imitation of fascist institutions.

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interests with national labels. When Cohen and Warwick rely on the official census for the ethnic composition of electoral districts and on votes among nationally identified political parties, therefore, they cannot measure an independent relation between regime strategies of political incorporation and varying political salience of ethnic identity any more than their evidence of increasing competition and ethnic fragmentation in the central ridge of the country can tell us what these votes represent. Partisan conflicts over economic policy, the collapse of world agricultural prices, depression, and protectionist barriers to immigrants in the West, all contributed to reducing economic growth, employment opportunities, and labor mobility—especially in this region of poverty and outmigration. Thus, there were few pressures to shake up territorially fixed, pre-1917 livelihoods or the influence of local political organizations on political identities.

The continuity of national discourse in the second Yugoslav state must also be understood from the perspective of mnemonic power and historical analogy rather than of the content of political institutions or the policies they aim to implement. This second state began, as did the first, with a wartime political pact among representatives of historical regions, its solonic will again emanating from those who controlled the army. It is difficult to imagine any aspirant to political power succeeding against the Kingdom or the Axis onslaught without the resources of the Communist Party: a centralized command, internal discipline, and preparation for armed struggle. (During the interwar period, this meant conspiratorial activities, survival of prison and torture without betraying comrades as a test of membership, as well as participation in the Spanish Civil War, and in 1941, the organization of the Partisan army.) Contrary to the argument that the Communist Party and its ideology was an alien force in Yugoslavia, the usefulness of Leninist doctrine and the number of Trotskyite militants in the interwar and wartime party attest more to the similarity in political conditions faced by Bolsheviks and Yugoslav communists than to the influence of the Comintern. The leaders of the Yugoslav Communist Party (CPY) were also conscious of the mistakes of the Serbian rulers, however, and took great pains to form a new, all-Yugoslav army. They timed their declaration of unification and of the principles for the postwar constitution to preempt foreign schemes being hatched by the Allied powers at Teheran, in order not to be caught in disagreement by international events (as the signatories at Corfu had been).[33] The party set up the base of the new government during the war—people's libera-

[33] See Vojmir Kljaković, "The International Significance of the Second Session of AVNOJ," *Socialist Thought and Practice* 23 (December 1983), 63-76.

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tion committees to provision the army and govern localities, regional political-military organizations based on party coalitions in which the communists strove for political preeminence, and a central war command composed of regional delegates originally sent into the field by the party leadership. The constitutive period was also preoccupied with the conditions necessary to all new states—negotiating international borders and alliances, subduing domestic opposition, and affirming the coercive monopoly of the party-state's army and police; but in contrast to the first state, the second defined enemies by class and collaboration.

Nonetheless, the repeated territorial threat to the lands of the South Slavs, and the construction of states on the basis of organizations built for defense, do perpetuate by analogy the association between institutions and national discourse. Mnemosyne's role is enhanced, moreover, by the ideology of the Yugoslav Communist Party. The analogy to Croat ideology is strong: the party claims its authority to rule as the sole representative of the political rights of a nation (working people) to self-governance[34] and as the army that liberated it from foreign occupation and the rule of the Serbian monarchy. Territorial rights may indeed have been secured by "Serb" institutions, as armies are treated in the language of national ideology, but the founding constitution of 1943 was republican and federalist.[35] The real link to the national question in the communist period is the very institution that, according to Banac, Cohen and Warwick, Burg, Ramet, and the Croat (and Slovene) program, was meant to solve it. The federal system gave national communities both property and voice after 1946, in some cases reuniting spheres that had been growing apart. As a result, the analogy with Croat ideology extends also to the current political system, for it internalizes conflicts similar to the tensions of corporate identities that are discussed above.

Territorial Defense, Budgetary Crises, and the Attack on State Power

The reforms of the 1960s and 1970s did not aim at national autonomy and Serbian power or at democratization of the party and freedom from

[34] *Samouprava*, more often rendered as self-management.

[35] Although much is made of this as the Croatian program, its full details are in fact closer to the Slovene constitutional proposal of 1921—suggesting, as does the entire path of political construction in the postwar period, the dominant influence of Edvard Kardelj, who was a member of the wartime leadership, head of the Slovene Party, and holder of many governmental positions, from Minister of Foreign Affairs to Vice-President. See Charles Beard and George Radin, *The Balkan Pivot: Yugoslavia* (New York: Macmillan, 1929). The Slovene role also serves as reminder that there is more to the Yugoslav national question than a Croat-Serb conflict.

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central dictate, although political debate frequently referred to both. They were not an exercise in legitimating communist power in the light of broken promises, nor were they the first change in the Yugoslav state; instead, they were part of an evolution that had begun in 1947. If we move from the ideological representation of those institutional changes to their content, then the far more mundane concerns of all states surface—the defense of territory and policies for economic development. Change in defense and economic policy implies change in public finance and employments. Because of the attempt to unite state, capital, and labor in Yugoslav institutions, these changes require adjustments in the jurisdictions and decision-making rights over economic allocations that institutions authorize; changes in the use of human labor—in the offices of state power and the economy—must also be made. At the same time, the choice of institutions is guided by the leaders' longer-term project (as national autonomists and Marxists) to dismantle the imperial state. This, too, is expressed in rules about public finance.

This alternative view of institutional reform in Yugoslavia will be illustrated in the context of the arguments made by the authors under review. The purpose of this contrast is to suggest the need for a new paradigm to study socialist states and the politics of reform that takes the content of their institutions more directly into account.

The economic and political reforms of the 1960s were a response to changes in the international configuration of power between 1953 and 1955; however, in order to understand the domestic process, some background on the country's complex property system is necessary. The federal system institutionalizes the historicist claims of "national communities" (the republics) to corporate ownership of the economic assets of their territory and to the allegiance of all who reside there, regardless of national identification. Federal rules and local governments guarantee the social and economic rights of individuals. The first set of institutions echoes Croat history, the second echoes Serb and Slovene. People employed in the socialized sector hold property rights collectively within their self-managed workplaces, but share them with territorial communities that legally own enterprises—which gives them the right to a rent (off profits or personal incomes) and an obligation to maintain and develop these assets.

Jurisdictional conflicts are somewhat relieved by a functional division of labor enacted in 1946.[36] Federal authority covers overall stability and

[36] See the speech before parliament on July 20, 1946, by the chair of the Economic Council, Boris Kidrič, "Obrazloženje Osnovnog Zakona o Državnim Privrednim Preduzećima" [Exposition of the basic law on state economic enterprises], reprinted in Boris Kidrič, *Privredni Problemi FNRJ* [Economic problems of the FPRY] (Belgrade: Kultura, 1948), 7-21.

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growth: external defense, foreign relations, the monetary system, and economic development. Republics develop their own economies—primarily light industries and agriculture, internal transportation networks, energy resources, regional development, as well as cultural affairs and secondary and university education. Communes are responsible for living standards—consumption goods, housing, local roads; they also guarantee constitutional rights for individuals—education, health care, pensions, and security for those with a permanent contract in a social-sector workplace, and ownership of capital assets within regulated limits for those in the private sector. Both forms of individual property are in fact defined by income: a guaranteed minimum in the social sector or market earnings in the private.

The state integrates these property holders into units of social decision making on the financing of social functions. According to the principle of social ownership, all assets are on loan from the community to those who work them, and these shared property rights are of equal authority and cannot be abrogated even temporarily without voluntary consent. The state (in Kardelj's reformulation of Lenin) thus becomes a set of councils for collective deliberation by representatives of the propertied: enterprises, communes, and republics. Representation is based on the right of those who create surplus value to decide jointly on what that surplus is and how it will be allocated among competing social purposes, and on stimulating their "interest" in its use. The management of funds either retained by producers or created for social functions, however, is an internal matter of property communities, whether national, working, or individual.

Both socialist and nationalist principles also prescribe an end to the state as an instrument of class rule or of national hegemony. Party leaders are committed to dismantle the state and progressively transfer its functions to governance by voluntary agreements between politically equal, self-managing communities, in accord with the improvement in material conditions. As early as 1946, the federal government was therefore denied the independent power base that comes from ownership of productive resources; any central extraction or coercion can only be temporary, and must be granted voluntarily by those who have such rights. This attack on state power occurs, in fact, through the transfer of its functions from budgetary financing to funds set up by assemblies of propertied representatives; they are then managed by those who perform the function. Social agreements are based on general policy lines set by the party; their implementation is to be ensured by the oversight and persuasion of party members who, as committed social activists, safeguard the principles of

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the system. To reduce the nonproductive drain of surplus value on administration, prevent the rise of a new ruling class, and protect the rights of internal management, these activists should be volunteers within their workplace, which defines their identity and income. As this process of what Yugoslavs call "de-statization" creates new self-managed communities, party units form within. To use April Carter's language for the opposite conclusion: as liberalization increases, so too will the role of the party in society.

Contrary to popular opinion, Yugoslav foreign relations were not determined by the break with the Cominform in 1948. During the period between the two reform movements that Carter identifies, there was a second struggle over foreign allegiances and their economic base. Activated by the end of the Korean War and the stabilization of Europe between 1953 and 1955,[37] a domestic dispute arose over defense strategy and the role of defense in federal expenditures: how to reduce its burden in favor of other needs that had been deferred in 1947-1952; whether to join the global move to nuclear-based weapons systems; whether to continue the partisan form of conventional defense; how to replace United States military aid that would end in 1957; and how to adjust to the abrupt reversal of Soviet military aid promised in the reconciliation of 1955-1956. In 1955, a major change in investment policy in favor of exportable light manufactures was aimed at increasing the role of both foreign commodity trade and imported advanced technology in domestic economic growth. Foreign exchange allocations were directed away from the military and toward export producers and foreign trade firms; civilian processing industries were freed to buy raw materials and machinery abroad on credit; and another agricultural reform socialized remaining uncultivated lands and consolidated holdings in order to increase production. The size of the standing army was cut, territorial defense was handed over to republics and localities, and a smaller central command structure was formed.

By 1958, responsibility for development policy had been transferred to the republics' economic jurisdictions. The decision to join GATT set in motion the two-stage reforms of 1961 and 1965 to liberalize foreign trade as required for membership. These policy changes were supported at home by economists and politicians opposed to the heavy defense burden and the economic interests it favored, by enterprises ready to benefit from greater autonomy over exports and imports, and by a variety of economic liberals. The creation, in 1958, of a European monetary system that ex-

[37] Including the creation of NATO, the final division of Germany, the neutralization of Austria, and the negotiations over Eastern security that led to the Warsaw Pact in May 1955.

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panded suppliers' credits for intrafirm trade with Western Europe had given economic impetus to the reforms; a second propaganda attack from the East—the Moscow Declaration of December 1960 and the Chinese critique—strengthened the political hand of the policies' domestic supporters.[38]

The policy debate did not end here, however. The GATT liberalization and the conditions attached to a standby credit from the International Monetary Fund to ease into reform and replenish foreign reserves led to rapid inflation and a serious recession. Angered by the Belgrade nonalignment conference of 1961, the U.S. Congress put an abrupt halt to further aid. Leaders divided by the policy shifts and their accompanying changes continued to quarrel while vacillating relations with the Soviet Union, the United States, and the nonaligned reflected the unsettled nature of the international system and Yugoslavia's difficulty in securing financing to cover continuing balance-of-payments deficits and to moderate the domestic recession. By 1966, however, the foreign debt had been reprogrammed, and Yugoslavia was a permanent member of GATT; it was conspicuously absent when the Warsaw Pact's political consultative committee met in July (the month Ranković was purged).[39]

To implement the new policies after 1958, firms were allowed to retain a greater portion of their earnings as an incentive to commercial profitability and a compensation for the cut in real personal income from higher prices and devaluation. Development funds and credit policy were transferred to republic-level banks, and multiple criteria for the flow of foreign exchange and investment credits were replaced by uniform rates. Carter calls this a marketizing economic reform. The reduction of federal economic jurisdictions and of federal revenues from foreign loans and tariff revenues also required a reform of the federal budget. Entire categories of federal expenditures—education, development, and defense—were shifted to self-financing (self-management) or to republic or local budgetary responsibility. Burg and Ramet characterize this process as federalization designed to reduce Serb dominance and to increase national equality. As the balance-of-payments deficit and inflation rose, policy makers responded with deflationary restrictions and a society-wide campaign of rationalization—incomes policies plus intensification of labor. The effect of enterprise mergers, further budget cuts, greater em-

[38] See Veljko Vlahović's speech to the Executive Committee (enlarged session) of the League of Communists on February 10, 1961, in *A Step Backward* (Belgrade: Jugoslavija, 1961), and Edvard Kardelj, *Socialism and War: A Survey of the Chinese Criticism of the Policy of Coexistence* (New York: McGraw-Hill, 1960).

[39] For details, see the chapter on Yugoslavia in Henry J. Bitterman, *The Refunding of International Debt* (Durham, NC: Duke University Press, 1973).

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phasis on technological modernization, and employment reductions was then counterbalanced by giving localities greater responsibility and "initiative" to protect peoples' jobs and incomes. All of the authors under review call this democratization.

Changes in economic organization in turn required changes in the organization of the government and the party. A constitutional commission began work in November 1960—at the time the Moscow conference attacked the party program that had been adopted at the Seventh Congress in 1958. The serious regional conflict of 1961 (though not the first, as Burg asserts) erupted over the meaning of these new policies for each of the republics. The divisive party executive committee meeting of March 1962 accepted the second stage of the foreign trade reform only after bitter quarrels (including a Slovene threat to secede from the federation, a major set-to between Serbia and foreign-trade liberals from Macedonia, and disputes with Ranković over who was responsible for maintaining the level of official salaries). Nonetheless, the new constitution, enacted in April 1963, affirmed the self-management of education and social welfare, with new chambers of the republic and federal parliaments; a reform of secondary education and university administration was to come in the mid-1960s, followed by the devolution to republic or local budgets of other federal tasks, such as road construction, territorial defense, and internal security.

Because more activities became self-managed and others devolved onto lower authorities, the party had to change itself. Republic and local party organizations would begin to finance their own staffs and therefore to appoint them; the federal cadre and control commissions thus became superfluous. This led to a debate, between 1962 and 1966, over the role of the party—not over democratization (as Carter argues), but over how to implement the changes: how to strengthen the party's role in enforcing societal principles if its base of operation lay within self-managed organizations; to whom party members would be responsible; and what influence the party should have on issues such as local investment decisions or technological rationalization that made employed labor redundant. Even more difficult was an old question from 1950-1952: in view of the simultaneous emphasis on development policy by the republics and increased mobility of economic resources in search of profitable investment, where should basic party organizations be located? To whom would the relevant party organization be responsible if an enterprise operated in more than one republic, and which should have greater authority locally—the enterprise committee or the commune committee?

The fact that party leaders called this a debate on inner party democ-

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racy reveals the extent of policy disagreement that remained in the highest circles. For the policies' proponents, party democracy meant that personnel changes were necessary to secure implementation: they wanted to remove potential opposition by placing people loyal either to the new ideas or to the reformers themselves (e.g., newcomers) in positions of authority, such as the federal secretariats for foreign trade and defense, the executive committee of the party, and major urban party committees. For the opponents, it was equally important to retain influence over the path of change, especially if their objections proved correct or conditions changed; party democracy meant freedom to remain in official positions and the right to dissent. This disagreement intensified the debate on organizational principles—how to secure the unity necessary for implementation if the instruments of party discipline were dispersed.

Top-level personnel changes occurred at the March 1962 meeting. But only when the diplomatic negotiations of the fall of 1963 sealed the commitment to the further liberalization of foreign trade and "territorialization" of defense in 1965[40] (with their consequences for international alliances and domestic policies), could a party congress be called to enact the new party statute and mobilize the troops. Delayed until December 1964, the congress set the stage for what was now labeled "the Reform," and for the constitutional amendments that followed.

Because all the works under review consider both the reform and these amendments to be decentralizing (increasing the republics' power vis-à-vis the center in order to restore legitimacy), it is worth noting the actual situation for republic and provincial governments. The shift to the republics' economic jurisdictions was a mixed budgetary blessing, particularly for those whose comparative regional advantage did not lie in activities earning foreign exchange, or those that produced raw and intermediate materials or had previously depended on supplements from federal development funds. By 1965, the republics' budgets and funds for development were fully independent of the federal budget and funds, and the general investment fund had been replaced (after great delay) by a fund for regional development, managed by regional representatives.

[40] See Anton Bebler, "Development of Sociology of *Militaria* in Yugoslavia," *Armed Forces and Society* 3 (November 1976), 59-68, for the dating of this policy. It was discussed again by the executive committee of the League of Communists of Yugoslavia in 1968, enacted in the Law on National Defense in February 1969, and publicly proclaimed at the Ninth Party Congress in April 1969. The doctrine of "general peoples' defense" was a return to the partisan war concept of total popular mobilization, on the one hand, and, on the other, a territorialization akin to the 1881 reform of the Habsburg army, which placed operations under the republics' military staffs, emphasized local postings, and aimed to integrate the army more fully into social and political life. See also Robert W. Dean, "Civil-Military Relations in Yugoslavia, 1971-75," *Armed Forces and Society* 3 (November 1976), 17-58.

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Federal subsidies to assist the republics in their legal obligation to subsidize communes whose per capita income was below the country average had declined significantly.[41] At the same time, deflationary policies since 1961 (including a moratorium on new investment and tighter credit) and greater enterprise autonomy over earnings (up to 70 percent by 1965) had cut republic and local revenues.

Representatives of the republics had been making federal policy since 1950. Each devolution of authority was accompanied by new central executive councils in which policies were defined and where central leaders could exert political discipline over the policies adopted. What changed after 1958 was not the influence of the republics on central policy or a greater activism bred of autonomy, but the nature of federal decisions and the stake that each republic had in their outcome. With federal investment resources gone, the principles of development assistance succumbed to the relative bargaining strengths among republics. Where once investment policy had been implemented by budgetary allotments or differential rates to accommodate multiple goals (as in tariff policy), the fate of the republics' economies and revenue bases now depended on uniform rules ("objective" in the debates) for allocating money. Within somewhat flexible budget constraints, multiple objectives that could earlier be made complementary became increasingly incompatible.

Thus, substantive outcomes depended ever more on what decision rules were chosen, and no one set of rules would benefit the republics equally, given the differences in their economies. Contrary to Burg's assertion that economic issues are "among the easier interbloc conflicts to resolve" and that the post-Tito leaders can succeed if they approach economic bargaining "pragmatically," following principles of "incrementalism," "reciprocity," and outside "ideological prescriptions" (p. 339), there is no optimal or nonpolitical resolution. Estrin shows that the institutional reforms of 1961-1965 *exacerbated* economic differences among republics, enterprises, and individuals—which is the reverse of Burg's argument. Nor is there a paradox, as Burg argues, in a more active parliament (especially the Chamber of Republics and Provinces) alongside a more assertive executive and proliferating extraparliamentary bodies for interregional and crossregional consultation. Because the republics with greater economic resources tended to win the battles on rules in ex-

[41] Slobodan Turčinović, "Financing Socio-Political Units, 1961-1967," *Yugoslav Survey* 9 (May 1968), 59-74. The proportion of national income expended by governmental budgets fell from 31.4% in 1961 to 25.7% in 1964 and to 18.9% in 1966; the federal proportion of spent budgetary revenues decreased from 54.4% in 1964 to 50.3% in 1966, or, when subsidies to republics are deducted, from 50.6% in 1964 to 45.5% in 1966. The share of communes rose from 27.6% in 1964 to 31.9% in 1966 (Turčinović, 73-74).

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ecutive bodies, those with fewer assets took their case to forums where monies could still be negotiated, such as budget debates in parliament and in organs managing special federal funds. The most public political battles among republics during 1965-1967 were over federal distribution of World Bank loans for road construction and over the criteria for development aid from the federal fund.[42] Because a republic's success or failure determined the salaries it paid to officials (of party, police, army, and government), its own power depended increasingly on federal decisions. The consequence of the new policies was to make real consensus impossible and compromise increasingly difficult. This, rather than some logic of balance between regional autonomy and central authority, was also the reason for the frequent changes in rules and organization.

Because the public side of policy consisted of bargaining between republics while the decisions by top leaders and influence of nongovernmental groups such as the Economic Chambers[43] or the I.M.F. were made behind closed doors, the republics' conflicts also provided a field day for the politics of historical analogy. The mnemonic power of national ideologies was particularly strong in 1967-1971 because the reform of secondary and university education had generated intense debates within the republics over educational policy—its national content, language policy, and the rights of minorities. What was in fact a complex of separate struggles over the altered property rights and privileges in these institutional changes came to be perceived by many, therefore, as a resurgence of Croat-Serb antagonism and nationalism.

The leaders of the republics were not averse to using this political resource in their struggles, as illustrated by the 1967 rule change from proportional representation of nationalities to equal representation of republics (and "corresponding" for provinces) in federal bodies. This was a campaign by the liberal leaders of Croatia to secure the economic reform against opposition within Croatia and control by republic governments over interrepublican financial flows—reminiscent of the contradictory liberal nationalism of the Yugoslavists. Since Serbs tended to predominate in the offices that were avenues to voting lists in poorer, less com-

[42] Slovenia created a political crisis because it did not receive the portion of IBRD funds it wanted; Bosnia took its dissatisfaction with the criteria set for development aid by the federal fund to the parliament, with constitutional consequences. These examples contradict Burg's division of the republics into two groups on levels of development and attitudes toward redistribution, as does the assignment of Serbia to the less developed camp (which is true in some ways but not in others, and does not take account of the strength of Serb liberals).

[43] Economic Chambers are sectorally organized associations of enterprises (previously the directorates between ministry and enterprise in the planning hierarchy) that also participate in making and enforcing policy; they deserve far more study than the preoccupation with the federal system accords them.

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mercially oriented Croat regions being marginalized by the reform, corporate representation of republics could silence Serbian voices in Croatia and cut their jobs without ever having to discuss policies. The rule also encouraged bloc voting in federal bodies. To gain greater control over the ministries of foreign trade and foreign affairs, the Croat leaders presented the issue as a contest between Croat autonomy and Serb expansion and alleged Serbian bias. (The ministries were held by associates of Ranković at the time.) In this way, they could gain allies among Slovene and Macedonian liberals and justify as democratic what was actually a move to cement their interests both at home and at the center. The rule change was a loss for Bosnia and Serbia, the most populous republics; for Muslims, who had been granted national status in the 1963 Constitution but no separate republic; and for the liberal leadership of Serbia, which was weakened by separate representation for Kosovo and Vojvodina. It also divided the liberal camp and encouraged Croat nationalists by playing on historical analogies.[44]

The change in individual rights and employments brought about by the policy shifts of 1958-1965 posed particular problems for a socialist state. Because recruitment to official and semi-official positions was through party membership, reformers had to effect a turnover in favor of policy supporters (and even non-party members whose skills were now prized) without losing the loyalty of those being shunted aside. The procedure had to be considered legitimate by all members of the political class. Since budgetary cuts, devolution, and rationalization inevitably threatened the income guarantee and employment tenure of those in social-sector jobs, a way also had to be found to maintain the living standard of those being dismissed without undermining the new budgetary and incentive policies.

As early as 1955, the army had attempted to maintain salaries, despite budget cuts, by encouraging officers to retire early and reducing citizens' military duties.[45] Rather than pay pensions out of local budgets, pension boards placed retired soldiers in civilian jobs, often in positions of high

[44] The same military border and marginal regional towns and a similar constituency—lawyers, schoolteachers, university students (but not the veterans)—gave early and strong support to the Croat cultural and patriotic organization Matica Hrvatska, which came to symbolize the nationalist "threat" in 1967-1971 while the official position of the Croat leadership echoed with early Yugoslavism; see fns. 18 and 23.

[45] Robert W. Dean reports that by 1972, of 437,709 veterans in Serbia only 32% were employed, while 66% were retired (about half of these were under the age of 55); in a 1971 national survey of the partisan elite "first fighters" (joining the partisans in 1941), more than half had been retired before pension age and 27% had incomes below the legal minimum and "complained most of social isolation, inactivity, and lack of prestige." Dean, "Civil-Military Relations in Yugoslavia, 1971-45," *Armed Forces and Society* 3 (November 1976), 17-58, at 54n.

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responsibility. Retirees also entered elections for local posts, such as the school boards created in 1955 by the first stage of self-management in education. By 1964, recalcitrant generals were being publicly pressured to retire, and three of the republics (Serbia, Bosnia-Herzegovina, and Macedonia) resolved local budget deficits by abolishing district governments entirely and consolidating many communes. By 1966, the removal of security police from budgetary rolls had to be legitimized by espionage charges against Ranković.

Although these middle-level functionaries, veterans, army officers, and security police had good reason to resent the policy that cost them their status and jobs, the political problem was not their programmatic opposition to the reform, but how to employ their time, pay their pensions, and provide the housing that had gone with their positions. Many of them turned to the economy for second careers. This increased competition with younger people at a time when new investment stagnated, unemployment rose substantially, and the traditional path of upward mobility through military, party, and government positions was closed.[46] Others turned to the new opportunities provided by competitive elections because success brought income, housing, and a restoration of social status to those who saw this as a continuation of their service to the country since 1941.[47]

Contrary to the authors' assumptions, the shift from central party appointment to regional jurisdiction, multicandidacy elections, and nationality quotas was not made to gain freedom from the party line, independence from central dictate, or real democracy; in fact, the reformers aimed at securing loyalty to the new central party policy with personnel changes made legitimately. When veterans and generals sent into early retirement from administration, and their campaign promises, proved popular in the 1967 and 1969 legislative elections (as unexpected and complicating as Ranković's popularity in the party elections in 1964), the re-

[46] Party finances are based primarily on members' dues, which are assessed on a progressive, proportionate scale of individual incomes. Thus, a bias is built in against those at the lower end of the income scale—young people starting out, rural youth, and blue-collar workers—both in the selection process and in the retention of members. (The first cause for expulsion, which affects many workers, is nonpayment of dues.) The more party finances were decentralized and put on an economic accounting basis, the greater would be this bias in poorer localities.

[47] Some victories, as one of Carter's election stories shows (p. 147), represented genuine disagreement on policies: in one Serbian constituency, a Partisan general unseated the official candidate, the federal secretary for trade, by disparaging his opponent's war record and his policy of permitting the import of cheap apples to the detriment of local fruit growers, by appealing to disgruntled peasants who had lost land in the agricultural reforms of 1958-1966, and by promising locals he would win their struggle for a railway line.

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formers shifted to single-candidacy elections and then to rotation of cadres to prevent alternative proposals from being aired within parliaments. Although Carter may be right in arguing that the radicals' proposals ultimately led to a multiparty system, it was their insistence on the right to speak freely, to debate party policy, and to criticize superiors without fear of dismissal, demotion, or involuntary transfer, as well as their growing opposition to the "unsocialist" outcomes of the reform and their influence among university students, that was the cause of their exclusion from the reform coalition.

Contrary also to the authors' argument, the explosion of political activity during the 1960s was not caused by institutional changes that reduced central power, Serbian hegemony, and political repression of national sentiments; the reasons for individual behavior are more easily traced to the effects of the economic policies on employment opportunities, wages, and social status. Estrin shows that the "robust and significant [statistical] relationship" (p. 4) between average earnings and relative market power, technical efficiency and scale, and input cost of firms meant an increase in income differentials, allocative inefficiencies, and the capital intensity of production.[48] Although Burg cites survey data to demonstrate the rise in nationalist feelings during the mid-1960s, the survey's respondents said growing economic inequalities were most important (pp. 46-47). Because wage rates are set within self-managed firms, however, workers' strikes occur there. Carter cites several cases in which Serb and Croat workers joined forces across republic lines against the policies of their republics' leaders, but we do not know the outcome because they fall outside normal channels.

Political activity was limited to those with acceptable organizational means. University expansion, for example, provided a public platform as well as a temporary alternative to unemployment. Cultural groups that demanded employment preferences on national grounds thrived in areas of mixed nationality, which tend to be poorer and more tied to domestic market production and administrative employment. Hence the irony that Kosovar demands for a republic separate from Serbia in 1967-1969 had to be formulated in terms of Albanian nationalism and anti-Serbism. Although no doubt fueled by a legacy of discrimination by Serbs, they make more sense as a reaction to the consequences of the very policies Ranković

[48] For the empirical evidence, see Estrin, chaps. 5 and 6. Estrin's interest is in testing some of the theoretical literature on labor-managed firms—"how firms will behave when the labour force plays the role of entrepreneur" (p. 1). He also argues—by indirection—that self-management explains allocative "imperfections" in Yugoslavia better than do the capital market theorists (the property rights school of Furubotn and Pejovich and the institutional school of Vanek and Jovičić).

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reportedly opposed than to his fall and a retreat of "his" security forces.[49]

According to these authors, the institutional changes of the 1960s were followed by another round in the 1970s to restore conservative party rule or to effect a better balance between central and regional power. Forced by the failure of liberal leaders in Croatia and Serbia to contain popular dissent, the new round began with the leaders' purge in 1972, but remained haunted by Tito's advancing age. But the explanation and timing more likely lie in the changes occurring in the international arena, their effect on the federal budget and its responsibilities, and the consequences that the policies taken in response had for collective and individual property rights . The policy of all-people's defense started to be implemented in 1968, with the Soviet invasion of Czechoslovakia providing yet another external rationale for the leadership. Many organizational changes "socialized" the army further—territorialization, devolution of control to localities and republics checked by a position in central policy-making councils, and closer integration of civilian and military lives. Kardelj's parliamentary speech at the end of 1967 made clear that "de-statization" was also entering a new phase; it was to culminate in the 1974 Constitution and the 1976 Law on Associated Labor, handing greater control over expenditure of surplus value to producers.[50] Although repayment of the country's foreign debt had been successfully postponed, at intervals, until 1969, it now concentrated attention on federal accounts. But this time, the international expansion of U.S. and European commercial banks offered the federal government a substitute for the export revenues of commodity trade, and new I.M.F. stand-by credits ushered in an era of foreign lending, stabilization programs, and rising world prices for raw materials and energy.

In the 1970s, leaders focused on federal-local jurisdictions, such as federal borrowing and incomes policies; political activity moved to the federal chamber of the parliament and to commune-workplace bargaining. Greater attention to domestic production of raw materials shifted the

[49] One reason the Albanian-Serb conflict in Kosovo is so intractable may be that Kosovar autonomists base their claims for territorially defined political rights (as they did in the interwar period against Serbia) on "historicist" grounds; but these are lands that Serbs also claim on historicist grounds—the rights of "primary acquisition" (the locus of the medieval Serb empire)—rather than on assimilationist principles. The Albanians' tactics to become the numerical majority in the region—first through their birth rate and later by harassing non-Albanians to move—may be seen as a predictable outcome of this ideological stalemate.

[50] The delegate system, by which parliamentary representatives at all three levels of government represent a "base" within a self-managed unit, was introduced in 1967 and has been a constitutional principle since 1974. Persons elected are already employed and in no need of supplementary (out-of-budget) income and perquisites. Self-managed interest communities (SICs) administer funds to provide social services that were transferred, with its bureaucracy, from a government budget to self-management; further financing is voted directly by enterprise representatives to a SIC assembly. The SICs' numbers grew exponentially after 1969.

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economic balance somewhat among firms and republics. The latter's bargaining over federal decisions grew more conflictual and prolonged because global stagflation sent contradictory signals to policy makers. The liberal leaders of Croatia and Serbia and their loyalists—now the obstacles to policy unity at the top—were forced to resign. The result, however, was not conservative recentralization, but further "de-statization."

The reforms continued in education and banking. More social services became self-managing. The party's presence grew in the military and in education. The republics gained veto power over federal budgetary expenditures, and their tax base moved back from personal to enterprise incomes. With the world recession of 1975, even the federal government's primary revenue source—customs duties—was transferred to self-managed funds of the republics, and firms were allowed to borrow in foreign markets. By 1979, however, changes in foreign financial markets and the country's still mounting debt renewed focus on commodity exports, and leaders returned to economic and constitutional reform. By 1986-1987, the severity of the economic crisis for individuals, the contests among leaders to implement new policies, as well as the losing battle for federal financing of an army no longer protected by Marshal Tito, all found expression in the bitter common language of national conflict.[51]

Conclusion

In attempting to analyze what lies behind institutional reform in this socialist state and to separate the ideological language of that reform from its substance, we have discovered a world of parallel and overlapping conflicts and of historical echoes that make interpretation extremely difficult. Institutional change in Yugoslavia follows a dual track: a Marxist project to create institutions whereby surplus value will be allocated by those who produce it, and a nationalist project to protect the rights of territorial power and a state-defined but open economy within a global system. The first, longer-term dynamic aims not only to eliminate the autonomous power of capital, but also to dismantle state power, and thus to end fiscal exploitation and bureaucratic rule. Government budgets and capital

[51] By the fall of 1988, the parallel with the disturbances of 1969-1971 was particularly striking, even though the alignment of republic leaderships and the issues for students and workers had changed. Now Serbian leaders (headed by Slobodan Milošević) were mobilizing popular nationalism in their bargaining for political reform, Slovenia was leading the fight over the foreign exchange regime, and Serb and Croat leaders were both neoconservatives. But the conflicts between proposals for economic and for political reform—those of liberal nationalism—have continued to divide the bargaining parties over the locus and criteria of fiscal and political (courts, police) regimes, even though they now agree on economic market reform. That a compromise has been negotiated without the presence of Tito illustrates the systemic character of these politics, and the role of international finance is far more visible.

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funds, and therefore the financing of both public goods and investment, have been progressively transferred to self-management—to government by the party and by producers (collective labor) in assemblies at the workplace and in territories. Simultaneously liberalizing and socializing, this process understandably echoes with the democratic language of early liberals and the proposals of autonomists under the Habsburgs and the triune Kingdom for national (rather than central) control over government budgets and the army, police, education, and even foreign affairs.

The consequence has been to confine the jurisdiction of federal institutions ever more to the nationalist project—territorial defense, liquidity in the balance of foreign payments, and regulation of the value of the currency and of the rules of monetary exchange that integrate territorial and workplace communities. The federal government, deprived of a base in production (unlike the communes and republics), must rely on foreign resources and alter rules on finance to influence the economy. Thus, the second, shorter-term dynamic of policy change and its institutionalization is (as in 1955-1967 and 1967-1979) not driven by domestic conflicts, but by changes in the military, trade, and financial policies of globally dominant powers to which Yugoslavia, as a small and nonaligned country, is essentially subject. Contrary to the presumption of self-management, however—that, by internalizing decisions on production and distribution and on policy and finance within property communities, conflicts can be resolved by discussion, compromise, and local initiative—some conflicts remain intractable. Leaders then jockey to change the rules and organization of financing substantive policies in favor of their constituencies, but still within the confines of the longer-term Marxist project.

Political cleavages are defined not by nationality or liberal/conservative party factions, but by the package of policies on credit, domestic and foreign currency, investment, and individual employment that coalesce around territorial defense and commodity trade. This line of conflict was formed before the changes of 1958-1967. It revives old ideologies about political organization: the conflicts echo old struggles, such as those between organizations for territorial control (armies, parliaments, police and courts), those for economic expansion (trade and processing firms, commercial banks), or the contradiction between liberal and national goals (schools and churches, markets versus governments). The system of self-management, ironically, perpetuates the mnemonic power of these institutional ideologies because it ties people to their working and residential communities; communal identities are not supplanted where labor and residential mobility is low.[52]

[52] Estrin argues (p. 2) that "labour market forces are weak in a self-managed system" ac-

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It is not surprising that pluralist, Weberian, and Marxian analyses of the socialist states continue to find material for their competing theses. The Yugoslav process of reform demonstrates, however, that the country's institutions are meant to combine the separate states they model into a new type. As this process continues, the logic of capital accumulation at the global level has had ever more influence on domestic policy, and the party has ever fewer autonomous resources with which to perform its governing role. What difference the remaining state institutions make in this task has yet to be studied directly.

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cording to both theory and his empirical data; the entry of new firms "effectively ceased" after 1964 (p. 90); mergers "generally occurred in a commune or neighbourhood" (p. 95); and overall concentration ratios began high and rose throughout the 1960s. As in the interwar period, declining labor mobility should be considered a significant factor in the popular responsiveness to national appeals, independent of whether their political expression is permitted or not. Similarly, when urbanization, social and economic mobility, and/or war undercut the residential bonding of communal (religious, occupational, racial) identities and their political expression in votes, loyalties, and collective action in three of Lijphart's main cases—the Netherlands, Lebanon, and South Africa—his predictions of stable rule through elite concert failed. See Lijphart (fn. 13).

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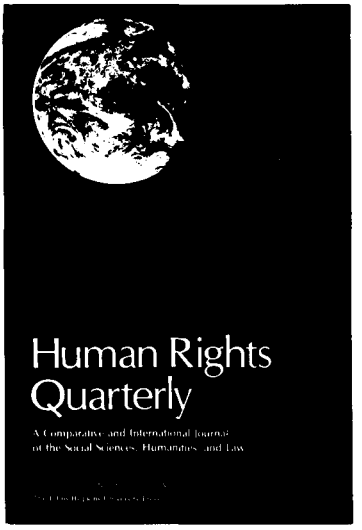

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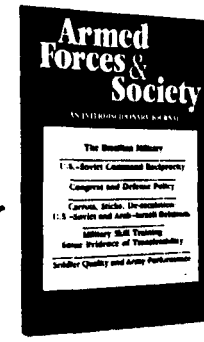

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